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THE ACTION OF STROPHANTHIN UPON THE CARDIAC TISSUE.

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Strophanthin has a very markedly specific action upon cardiac tissue, and this is shown by the drug, when injected into an intact frog, stopping the heart in systole, before any other organs are affected. The same specific action can be demonstrated upon the isolated tissues of the frog, the heart being killed by concentrations that are too low to produce any effect upon other tissues.[2]

This selective action might be due to strophanthin entering into chemical combination with some constituent of heart muscle. The isolated hearts of frogs and mammals, therefore, were perfused to see if the heart tissue retained the drug; but in no case was there any certain evidence of the drug being either retained or destroyed by the heart; in particular it was shown that the frog's heart retained less than 0.0008 mg. of strophanthin.[5] Straub had previously obtained a very similar result.[12] No strophanthin ever could be recovered from the isolated hearts of frogs or mammals after perfusion, but small quantities were demonstrated in the snake's heart after perfusion with high concentrations of strophanthin, but in all cases the concentration of strophanthin in the heart was much lower than that in the perfusion fluid.[5] There is therefore no evidence of any specific absorption of strophanthin by cardiac tissue; moreover, the quantity of strophanthin retained in the heart is so small that it appears probable, as Straub suggests, that strophanthin exerts its action upon the surface of the heart cells.

Tolerance to Strophanthin.

Certain animals show a well-marked tolerance to strophanthin, and the isolated organs of these animals were investigated to determine the cause of this tolerance. Comparative experiments made with atropine confirmed the conclusions of earlier workers, that tolerance to this drug was due to the power of the liver to absorb and destroy it,[3 4] and the same cause has been shown to account for the tolerance of certain animals to nicotine (Dixon) and morphine (Faust). The rat and the grass snake were found to show a natural tolerance to strophanthin, but the organs of these animals had no power either to absorb or to destroy the drug.[5] Hatcher[9] also showed that the intact rat, when given strophanthin, excreted the bulk of it unaltered. Gunn[8] found that the isolated heart of the rat showed a well-marked tolerance to strophanthin, and the writer found the same with the isolated heart of the grass snake.[6] The tolerance to strophanthin appears therefore to be of the nature of a tissue immunity, for the tissues upon which the drug acts are, in tolerant animals, less susceptible to the action of the drug. This variety of tolerance is rare, for in most cases tolerance is due to defensive mechanisms, which prevent the drug from reaching the tissues upon which it acts, and in such cases these tissues upon which the drug acts are equally susceptible in tolerant and non-tolerant animals.

The action of strophanthin, therefore, is extremely specific, for it acts only on cardiac tissue, and the intensity of its action upon this tissue varies greatly in different animals. It has also been shown that little, if any, of the drug enters the heart cells, and that therefore it probably acts upon the surface of the cells.

The Relation between the Action of Strophanthin and the Action of Certain Ions upon the Heart.

The action of strophanthin was found to show a resemblance to the action of alkalis upon the heart. The snake's heart was more tolerant than the frog's heart to strophanthin, and also to the action of acids and alkalis,[6] but both hearts were affected equally by other systole-producing agencies (barium and saponin). Moreover, the action of strophanthin upon the frog's heart was found to resemble that of alkali in many respects. Therefore the effect of alterations in the ionic content of Ringer's fluid upon the frog's heart was investigated in order to compare these with the effect produced by strophanthin.

Action of Ions upon the Frog's Heart.

The following summary is based upon the work of Mines,[11] and on certain unpublished results obtained by the writer. The heart's activities can be considered most conveniently under the two aspects—the conduction of excitation and the contractile process; and both of these processes are nearly at an optimum in a fresh heart perfused with normal Ringer. Both the rate of conduction and the rate and force of contraction are reduced by the following changes—namely, an increase in the acidity, or in the potassium or the sodium content of Ringer, or by a decrease in the calcium content; and these may be termed depressant changes. The reverse changes—namely, increase in the alkalinity, or a decrease in the potassium or sodium content, or an increase in the calcium content of Ringer—may be termed stimulant changes; they produce, however, little beneficial effect either upon the rate of conduction or the force of contraction in the fresh heart.

When a heart was exhausted by prolonged perfusion with Ringer (hypodynamic heart), then stimulant changes produced a well-marked increase in the force of contraction, but did not improve the rate of conduction, although this was greatly below the normal in the hypodynamic heart. The depressant changes impaired both the rate of conduction and the force of contraction more markedly in the hypodynamic than in the fresh heart. The difference in the action of ions upon conduction and contraction is of interest, and Mines has shown that a lack of calcium diminishes the force of contraction of the heart much more rapidly than the rate of conduction of the electrical wave of excitation; he concludes that the two processes may vary independently, and that the function of calcium is much more intimately associated with the contractile process than with the process of conduction.

The cause of the hypodynamic state next was investigated, and it was found that the heart during perfusion lost a lipoid-like substance, and that the addition of lipoids, or soaps of the higher fatty acids, produced a very marked beneficial effect upon both the rate of conduction and the force of contraction in the hypodynamic heart. From these facts it was concluded that the hypodynamic state was due to a loss of lipoids, and that the activity of the heart was dependent upon the presence of calcium and lipoid, probably in the form of an insoluble compound, at the surface of the heart cells.

Action of Strophanthin.

The action of strophanthin upon the heart is represented by Figs. 1 and 2 (p. 898). The action can be divided into two stages—a first or therapeutic stage, in which the force of contraction of the auricle, and to a less extent that of the ventricle, are increased, and a second or toxic stage, in which the force of contraction is diminished, the rate of conduction is impaired, and the diastolic relaxation is imperfect. The action of strophanthin upon the hypodynamic heart is shown in Fig. 3, and it will be seen that the increase in the force of the ventricular contraction is much greater than in the fresh heart; this difference suggests a possible parallel to the difference in the action of cardiac glucosides upon the healthy heart and the diseased heart in mammals.

The Action of Strophanthin when the Ionic Content of Ringer is Altered.

The effect of various alterations in the ionic content of Ringer upon the action of strophanthin upon the frog's heart was investigated, and it was found that the systolic effect was opposed by the presence of acid, or by the

Fig. 1.—Action of strophanthin upon the recently excised frog's heart. The upper tracing shows the auricular and the lower the ventricular movements. Read from left to right. Upstroke systole. Time in seconds. (See Table I.)

TABLE I.

Time.	Rate.		Output per Beat in c.cm.	Length of *a–v* Interval in secs.
	Sinus.	Ventricle.		
Start ...	28	28	0.080	0.3
1 min.*				
10 ,, ...	30	30	0.090	0.33
20 ,, ...	26	13	0.077	0.4
30 ,, ...	22	8	—	—

* Strophanthin 0.0001 per cent. perfused.

hypodynamic condition, and also that systole could not occur in the absence of calcium. The writer also agrees with Straub's conclusion—that the concentration of strophanthin that will produce an impairment of conduction is lower than that required to produce a well-marked systolic action. These results will explain the so-called diastolic action of strophanthin upon the frog's

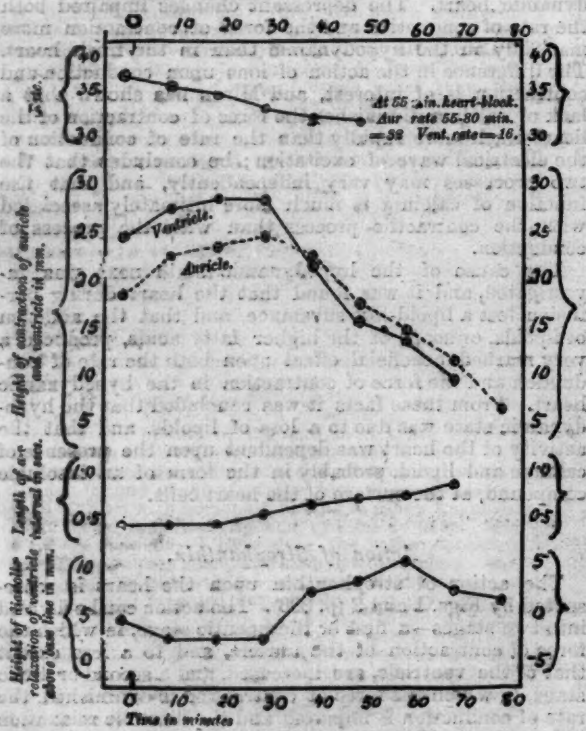
At 55 min. heart-block.
Aur. rate 55-80 min. = 32 Vent. rate = 16.
Ventricle.
Auricle.
Height of contraction of auricle and ventricle in mm.
Length of a-v interval in secs.
Height of diastolic relaxation of ventricle above base line in mm.
Time in minutes

Fig. 2.—Action of strophanthin upon the freshly excised frog's heart. Strophanthin 0.00005 per cent. perfused at 1 minute.

heart described by certain writers. The writer found that the action of strophanthin in improving the force of the heart's contraction, and also in producing systole, showed a very close resemblance to the action of alkalis upon the heart.

The action of strophanthin in diminishing the rate of conduction shows no such resemblance. The experiments of H. Straub,[12] who obtained records with the electro-cardiogram of frogs' hearts poisoned with strophanthin, show that the rate of conduction of the electric wave of excitation is decreased by strophanthin, and therefore that the delay in the occurrence of the ventricular contraction is due to interference with conduction, and not to an increase in the latent period of the ventricle.

The action of strophanthin upon conduction appears to be of a different nature to its systolic action, for it occurs

Fig. 3.—Action of strophanthin upon the hypodynamic heart of the frog. The heart has been perfused for 6 hours with Ringer's fluid. The upper tracing shows the auricular and the lower the ventricular movements. Read from left to right. Upstroke systole. Time in seconds. (See Table II.)

TABLE II.

Time.	Rate.		Output per Beat in c.cm.	Length of *a–v* Interval in secs.
	Auricle.	Ventricle.		
Start ...	32	16	0.10	0.8
1 min.*				
10 ,, ...	24	12	0.14	0.9

* Strophanthin 0.0001 per cent. introduced.

with lower concentrations of the drug than does the latter, and also is not antagonized by alterations in the ionic content of the Ringer.

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THE eleventh congress on medicine as a profession was recently held by Belgian doctors at Ghent under the presidency of Professor Van Duyse of Ghent and of Dr. Herman, President of the Belgian Medical Federation. A resolution was passed vehemently repudiating the notion of the socialization of medicine in the name of the independence of the medical body, of the interests of science, and of the public interest. It was further resolved that there never has been opposition between the material interests of the doctor and preventive medicine, the profession having hitherto taken all the initiative in the developing of public hygiene. The congress called on all doctors concerned with the administration of public benevolence, to mutual aid societies, and to collective organizations of all kinds, as well as on isolated practitioners, to take an active part in the work of preventive medicine by making themselves the sanitary counsellors of administrations and societies. The view was expressed that in places where several doctors reside it would be convenient to create groups for the purpose of dealing with questions of general and local hygiene. These groups should keep themselves in direct and constant touch with the authorities in regard to all public questions relating to medicine—school inspection, prophylactic measures, the creation of dispensaries, and so forth. It was urged that all doctors should avoid isolated action in matters affecting the interests of the profession.

A Clinical Lecture
ON
MAXIMAL AND MINIMAL BLOOD PRESSURES AND THEIR SIGNIFICANCE.

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Within recent years and in many countries considerable interest has been taken in blood pressure. Much has been written and many sphygmomanometric instruments have been devised. For general purposes, these instruments, as I have shown in a previous paper,[1] may be grouped into two divisions, according to the main principles of their construction, the first division comprising those forms of apparatus founded upon the method of estimation of the pulse wave below the level of compression, whilst the second includes those based upon estimation of movements or oscillations of the arterial wall at the level of the compressed portion of the limb. As an example of the first group I will take an instrument of the Riva-Rocci type; as an example of the second group, the sphygmo-oscillometer of Pachon.

In any branch of experimental science not only is it advisable, but indeed essential, every now and again to call a halt in order that we may review our progress and position in the light of recent knowledge, while at the same time we assess and reassess as carefully as possible both the accuracy and extent of the groundwork upon which our superstructure rests. And it is for this very reason that I speak on this subject to-day, for, in my judgement, the greater part of the clinical work on arterial pressures hitherto attempted is founded on a basis, true so far as it goes, but which represents only a part of the whole truth.

First of all, what do people generally mean when they speak of blood pressure? In this country, when blood pressure is referred to, the radial systolic pressure registered by some or other modification of the Riva-Rocci sphygmomanometer is usually meant. Indeed, from the way in which many speak of "blood pressure," it would appear that a certain blood pressure is as characteristic of an individual as the colour of his hair or the shape of his nose. But this is not so, for although the blood pressure of a healthy individual may in general be confined within limits which possibly differ but little from those of his next-door neighbour, yet such pressure is not constant from month to month or from day to day, but from respiratory, psychical, or other causes may vary even during the time of investigation. In some subjects blood pressure is far more stable than in others, and observations over long intervals of time may approximate to an equal series, characteristic for each under like conditions. But this approximation is never absolute, for in reality blood pressure is a quantity capable of variation with each heart beat, with each change in tonus of vessel wall, with the peripheral resistance, and with the total volume of circulating blood. For an individual, in short, arterial pressure has no fixed value. It is always fluctuating and never isodynamic. Hence the maximal and minimal pressures between which arterial pressure normally varies are the important criteria which in every case we must definitely determine.

In a physiological laboratory the pressures within an opened artery form the usual subject of investigation, and, in any comparison between laboratory and clinical methods, it is necessary to bear in mind this fact, for the reason that, while such physiological methods usually record only the mean blood pressure, our clinical methods never measure the mean pressure, but either the maximum pressure (systolic), or the minimum pressure (diastolic), or both, according to the kind of instrument employed.

Those who are accustomed to work only with sphygmomanometers of the Riva-Rocci type urge that registration of the diastolic pressure is a needless refinement, and that for all practical purposes a record of the systolic pressure suffices. Further, it has even been stated that the "diastolic" pressure can have no true significance, since the pressure during diastole is continuously falling. In like manner, would it not be just as reasonable to ignore the "systolic" pressure, since during systole there is a rising pressure? In fact, such statement appears to show an entire lack of appreciation of the terms involved in practical sphygmometry, for, according to the accepted terminology, the pressure called "systolic" is that pressure which is recorded at the highest point of the systolic crest—in other words, the *maximal* pressure—while the "diastolic" is that pressure which is taken at the lowest point of the diastolic curve—in other words, the *minimal* pressure; and to intervening pressures, including the mean pressure, the terms "systolic" and "diastolic" are not without qualification applied. Pressures between the height of systole and the depth of diastole should properly be termed "interpressures," and although a knowledge of these is desirable, yet, at the same time, they should not be taken as criteria in blood pressure observations.

From the evidence which I now proceed to lay before you I think that you will agree that the above objections are mistaken ones, and that to record the maximum pressure whilst despising the minimum not only affords no indication of what the mean pressure is likely to be, but resembles attempting to solve a complicated problem of which one factor only is given. "No fact," says Janeway,[2] "regarding the blood pressure is better established than its wide range of variation in any individual. It is therefore impossible to speak of a normal value for blood pressure, but only of certain normal upper and lower limits."

Once we have succeeded in establishing the truth of this proposition, self-evident indeed but till of late unrecognized, we are confronted with another point, equally clear and direct, the importance of which in this country is only now becoming recognized, namely, the diastolic pressure. Marey[3] was the first to demonstrate as the criterion of the true diastolic pressure the external pressure at which maximum amplitude of the arterial wall becomes manifest, and the truth of this great and fundamental observation has since been verified by numerous workers. At the Annual Meeting of the British Medical Association in 1911, in reference to the use of Pachon's oscillometer, I said, "Now that we are in a position to institute a comparison between the maximal and minimal pressures, it will probably be found that of the two the *diastolic* is the more important."[4] Additional experience during the past two years has served to confirm the belief that I then expressed. And why do I say this? Chiefly for the reason that the diastolic pressure is a charge which the arteries must constantly bear, and from which they cannot escape. There is no point below which the *minimal* pressure can fall. The diastolic pressure, therefore, is a constant charge, whilst the systolic pressure represents only an intermittent supercharge.

Now let us examine in greater detail what is meant by the kind of statement so frequently made that a given blood pressure is found to be, say, 120 mm. Hg. This is usually taken to indicate that a distensile bag with outer non-elastic cuff has been placed around the arm above the elbow, and that air has been pumped into the bag until the pressure within it has been raised to height sufficient to obliterate the pulse at the wrist. The pressure in the bag is given either by a mercurial or aneroid manometer, and, in the example just given, the observer assumes that the pulse becomes extinguished when the manometer reading is 120 mm. Hg.

What has actually been recorded is simply the fact that in a given individual at a particular moment a pressure of 120 mm. had to be applied around the arm in order to cause the pulse at the wrist to disappear. From other evidence, the nature of which I need not now enter upon, it is assumed that this pressure is equal to the systolic pressure in the radial artery. Were the arteries rigid tubes like gas or water pipes, and were the blood a stationary fluid whose pressure was not constantly fluctuating on account of the beat of the heart between a systolic maximum and a diastolic minimum, by the ordinary laws of hydrostatics the pressure would undoubtedly be the same at every point. But the blood has dynamic energy,

it is in constant movement, and the arteries are not rigid tubes which serve merely as conduits, but being themselves extremely distensile and elastic tubes exert a powerful influence upon the blood flow. We all know that in the arteries the pressure is high and intermittent, while in the veins the pressure is low and continuous. The intermittent flow in the arteries when passing through the capillaries is broken down into a continuous flow in the veins. Where does this breaking down occur? Are we to assume that the systolic pressure is the same from the aorta through the brachial into the radial artery, and that it is not until the blood stream arrives at the capillary area or just before the capillary area that the intermittent pressure is broken down into a continuous one in the veins? Or, alternatively, is the pressure being continuously broken down by the arteries throughout the whole of the course from aorta to capillaries? If the latter be the true explanation, the systolic pressure should be higher in the aorta than in the brachial, higher in the brachial than in the radial, and so on. Accordingly we should speak, not of *the* systolic pressure, but of the brachial, radial, femoral, etc., systolic pressure, as the case may be. What really happens with the Riva-Rocci type of instrument is that the observer compresses one artery, the brachial in the arm, whilst he takes as the systolic pressure the disappearance (or return) of the pulse in another artery, the radial in the forearm. Quite apart from disadvantages due to personal differences in tactile sensibility, inertia of the mercury column, etc., the fact is that the compression is exercised, not at a point which coincides with that at which the pulse is felt to disappear (or return), but at a different level altogether, and I can easily show you that when the pulse has already disappeared in the radial artery the brachial artery still shows ample pulsations.

Leaving this point for a moment, from another aspect I wish to direct your attention to the inadequacy of considering the systolic reading alone. Supposing the interval between the top of the systolic crest of the pressure wave and the bottom of the diastolic notch were always the same, it would not matter which reading we took, and we might then safely rely on systolic pressures alone. As it is, however, merely to state this assumption is sufficient to show its falsity. We all know that the size of the pulse—the amplitude of the pulse curve—varies enormously in different individuals, and indeed in the same individual at different times. In other words, the relation between the systolic maximum and the diastolic minimum varies. Let me give you a supposititious illustration: In one man the systolic pressure in the radial artery is 140 and the diastolic pressure is 80; in another the systolic is 120 and the diastolic is 100. Which of the two has the higher blood pressure? In which is the artery undergoing the greater strain? If we are content to record systolic pressures alone, unquestionably we should say that the man with a systolic pressure of 140 ran the graver risk. If we regard the diastolic alone, we should say that the artery of the second individual was exposed to the greater strain, but if we take the arithmetical mean, assuming for the sake of argument that the mean figure between the two extremes represents the mean pressure, we see that the strain is equal in the two cases. Now, as a result of investigations extending over some considerable time, I have found in many cases, and especially in aortic regurgitation which affords some of the best and most striking examples, that an abnormally high systolic pressure is accompanied by an abnormally low diastolic pressure. Hence to record systolic pressures alone is fallacious and misleading.

Let me give you as illustrations two actual cases:

A certain patient of mine suffering from aortic regurgitation gave as his systolic pressure 210 mm. Hg. Another man—a case of granular kidney—gave a systolic pressure of 180. Had I regarded the systolic pressures alone I should have said that the aortic case had the higher blood pressure, and that his arteries were in a condition of greater stress than those of the man with granular kidney. But a record of the diastolic pressure in each case put an entirely different construction on the matter. The diastolic pressure of the aortic case was 70 mm. Hg, whilst that of the renal case was 140—that is to say, the renal case had his arteries constantly kept on the stretch by a minimal pressure of 140, which during systole rose to 180; whilst in the aortic case the diastolic pressure was only half this amount, and it was solely during the brief interval of time represented by the upper part of the sharp systolic peak that the pressure reached a notable elevation. During diastole the arteries were far less stretched than normal. Various other considerations have convinced me of the futility of taking only the systolic pressure, which I do not propose to enter into here, as I think enough has been said to show that any argument based upon systolic readings alone must be received with the very greatest caution.

If under pathological conditions the interval between the systolic maximum and the diastolic minimum can vary to degrees even more extreme than shown by the cases I have just cited, the question naturally arises, May there not be from time to time physiological variations in the same individual? Is it not possible, for instance, that the systolic pressure may rise and the diastolic fall, so that while the mean pressure remains constant, either a systolic or diastolic reading, if considered separately, would show a variation in pressure? What I regard as an extremely important contribution to our knowledge of the subject has recently been made in a paper read before the Royal Society by Dr. Russell Wells and Professor Leonard Hill[5] on the influence of the resilience of the arterial wall on blood pressure and on the pulse curve. It is possible that I am attaching more importance to this than it deserves, since I am partly responsible for the work on which it is based, and shall shortly, in conjunction with others, publish this evidence in full, so that it can be criticized and confirmed or refuted. Nevertheless, as I personally believe it to be true, I feel it only right in discussing this subject to give you the outlines of the view in question.

Up to the present time it has been assumed that arterial elasticity is a fixed quantity for each artery, and does not vary. The suggestion is now made that the arteries have the power of becoming more or less rigid, less or more resilient from time to time under the influence of the nervous system, or from other causes. In order to be clear, perhaps it would be well to explain my meaning at greater length, and if I seem to some of you to be talking rather elementary physiology, I trust that you will forgive me, as I find it rather difficult adequately to explain this idea without some simple illustrations. The word "resilience" is used to express the ease with which an elastic tube distends with a rise and recoils with a fall in pressure of the contained fluid. In this sense a glass tube possesses no resilience. A rubber tube with a wall 0.2 mm. thick is more resilient than one with a wall 0.4 mm. thick. The thinner, more resilient tube yields with a rise and recoils with a fall of pressure to a greater extent than does the harder and thicker-walled one. If the arteries were rigid tubes, and if the systolic pressure generated at each beat of the heart were, say, 140 mm. Hg in the aorta and the diastolic 60, both systolic and diastolic pressures would be constant throughout the arterial system and at the threshold of the capillary area. But the arteries are not rigid tubes; they are resilient, and consequently their walls become distended during systole and recoil during diastole. Hence, further down the arterial tree we shall find the systolic pressure lowered, say, to 130 mm. Hg, and the diastolic raised, say, to 70. Were the resilience of the different arteries a fixed quantity for each, the amount by which the systolic pressure would have fallen and to which the diastolic would have been raised would be always the same for the same pressure differences at the heart. But the vasomotor tonus is constantly changing in various portions of the arterial system so as to meet the local requirements, and if for any reason a particular artery becomes more resilient, the systolic pressure within that artery will be further reduced and the diastolic more raised.

Even this assumption, however, is not unchallenged, for Russell[6] of Edinburgh declares that some of the pressure in the sphygmomanometer bag is used up in overcoming the resistance of the vessel wall. According to him, therefore, the systolic pressure in the radial artery in our example would be 120 minus x mm., x being the amount required to cause the vessel wall to collapse. Others, again, have maintained that x is a negligible quantity, and that 120 would really represent the radial systolic pressure. I am not at present concerned in discussing this

point or the other assumptions that underlie the statement that the method I have outlined gives the true systolic pressure in the radial artery. I do, however, wish to point out that all that can be asserted is that the systolic pressure was 120 mm. in the artery observed at the moment of observation.

From the fact that in healthy young adults in the recumbent posture the systolic pressure in the posterior tibial artery is found to be about the same as in the radial when similarly measured, there seems to have arisen a supposition, tacit perhaps rather than avowed, that the systolic pressure in all the arteries of the body would be much the same. This is a very large supposition, and, as I believe, an erroneous one.

Having already demonstrated that a record of systolic pressure is of slight value in the absence of a simultaneous record of diastolic pressure, we next have to ask, What are the instruments which afford the information that we seek? Instruments recording both pressures are few in number, and of these the best are the sphygmomanometer of Erlanger and the sphygmo-oscillometer of Pachon. For graphic records of physiological blood pressure in suitable cases Erlanger's apparatus has considerable utility. In clinical medicine, nevertheless, its employment is contra-indicated, since it is too complicated, cumbersome, and expensive for purposes other than those of the laboratory.

No such objections apply to Pachon's sphygmo-oscillometer, which is less expensive, easily portable, and simple in application. Moreover, if properly used, it eliminates the personal equation, so that, having now used this instrument for upwards of three years, and having tested it against a large number of other apparatus, I am able to say that with it I have obtained very useful clinical results. By this I do not mean to say that I consider it a perfect instrument, but it is certainly quite a good one, and both in private work and in hospital I use it in preference to any other. One minute only is required for an observation, which, however, I always repeat for the sake of accuracy, and at the end of this short time one has a complete record of maximal pressure, minimal pressure, and amplitude of pulse wave.

In stating that the Pachon sphygmo-oscillometer gives a record of maximal and minimal pressures, I do not say that even these figures represent the real blood pressures of the case under observation. More investigation on this point is required than I have yet been able to make. The salient fact is that both pressures are obtained simultaneously by the use of the *same* method, and that these are relative one to the other, thus affording exceedingly valuable clinical comparisons over large numbers of cases. It is of no avail to measure one pressure by a mercurial manometer, for example, and another by the method of oscillations, for readings of the systolic pressure by one method and of the diastolic by another method totally different in principle fail to give results which are sufficiently accurate to be comparable with advantage. In fine, my opinion is that an absolutely satisfactory means of recording at the bedside the various blood pressure data has yet to be discovered.

To a future occasion I must leave the remarks which I had hoped to make on readings taken in the manner I have described as guides to prognosis and treatment. It has been denied that blood pressure records can give any useful indications in these respects; but, again, this pronouncement has been made from consideration of systolic apart from diastolic pressures. Personally, I may say that I often find readings taken at frequent intervals of considerable assistance when considered in conjunction with the other available physical signs and symptoms. For instance, if in a case of arterio-sclerosis both pressures are rising in spite of treatment, the import of this is unfavourable. In a patient of mine, a lady aged 65, the subject of hypertrophy of the left ventricle, generalized arterio-sclerosis, and some chronic interstitial nephritis, the arterial blood pressures, systolic and diastolic, have been throughout the past two years very high. Some nine months ago the pressures rose, and I considered her to be in danger of cerebral apoplexy. This event speedily happened, being associated with paresis of face, arm, and leg on the same side, from which now the patient has made very fair recovery, and the blood pressure has become slightly lower than the original figures. As regards treatment, the case of a schoolboy, aged 14, with well-marked double aortic disease, is of interest. On his first visit the readings were 20—2—8, the first being the systolic pressure in centimetres Hg, the last the diastolic, and the intermediate figure the amplitude of pulse wave. Three weeks later the readings were 14—0.5—7, the general condition and state of the circulatory system having correspondingly improved.

It would be easy to multiply examples of general diseases as well as of circulatory affections, but, since I hope to deal with these in a later paper, I will conclude with the recommendation that, as opportunities present themselves, you will test for yourselves the importance of registering diastolic as well as systolic pressures.

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THE IDIO-VENTRICULAR RHYTHM.

A CLINICAL CASE WITH POLYGRAPHIC RECORDS.

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The term "bradycardia" is applied correctly to those cases of infrequent heart action in which the auricles participate in the infrequency. It may be either normal or of the so-called nodal type.

In the normal bradycardia the auricular contractions bear the normal time relationship to those of the ventricular. The term "nodal bradycardia" is applied to that form in which the auricular contractions occur simultaneously with, or very slightly before or after the ventricular contractions. The case on which this paper is based may be regarded as one primarily of bradycardia, and it has some features in common with nodal bradycardia as defined. It differs, however, in this, that the polygraphic records show a regularly-recurring auricular contraction to which the ventricle fails to respond.

In cases in which the conduction of impulses is not interfered with, the idio-ventricular rhythm asserts itself, provided the period of time occupied by the rhythmic formation of impulses is less in the ventricles than in the auricles. So that a lengthening of the time of stimulus formation at the sinus, or a lessening of the time in the ventricles, or a combination of these, will produce the assumption of the idio-ventricular rhythm.

In the case of bradycardia we have, then, an essential element for the production of the idio-ventricular rhythm, provided the bradycardia is sufficiently pronounced, and the period of time occupied by stimulus formation in the ventricle is not also lengthened sufficiently to prevent the ventricular rhythm from coming into action.

A Case Illustrating Idio-ventricular Rhythm.

On December 5th, 1912, a patient came to me complaining that he was unable to continue his work because of pains in the chest, arms, and legs, and some breathlessness. He is a blacksmith by trade, aged 32 years. He has been at this trade for the last ten years, but previously to that from the age of 16 years had been employed in a coal mine. He is a small man and not of good muscular development.

There is no history of illness or injury likely to cause affection of the heart. He denies having had syphilis. He has had no attacks of fainting.

He has a troublesome cough, with some expectoration.

The pulse is small and at the rate of 40 per minute, and is almost regular in rhythm. There is no evidence of atheroma. The heart sounds are clear but faint, and they correspond to the beats of the radial pulse.

The lungs are emphysematous, which prevents the size of the heart being determined. There are no signs in the lungs to account for the cough. There is some tenderness and increased muscular resistance in the region of the liver.

There is no oedema of the extremities or other signs of heart failure other than those mentioned.

The urine does not contain albumin.

On December 5th, 10th, and 16th tracings similar to that shown in the figure were taken. In the venous curve of the figure the *a* and *c* waves occur almost simultaneously, resulting in a rise with two peaks. Measurement shows that the first of the two peaks is due to systole of the ventricle, and it is probable that the second is due to auricular contraction, though it must not be lost sight of that it may be due to instrumental error. The whole wave is, however, too broad to be entirely due to ventricular contraction. The combined wave causes a large excursion of the recording pen, especially when compared with the radial pulse wave. This is usual in cases when the *a* and *c* waves fall together. It was

difficult to obtain a record of the radial pulse wave at all, as the wave was so feeble. These larger elevations due to the *a* and *c* waves follow at regular intervals, and are succeeded by smaller waves, *v*, which measurement shows are due to filling of the auricle, the fall of the wave being coincident in time with the opening of the auriculo-ventricular valves. Following the *v* waves is in many instances a sharp though small rise in the tracing, followed by a very small fall. This small wave is most likely due to the closing of the auriculo-ventricular valves, due to the inrush of blood into the ventricle (the *b* wave of Gibson). After this wave comes another (marked *a'*) not very pronounced, but which is nevertheless distinct and frequently present.

A great deal of the interest of these tracings centres upon the interpretation of this wave. It recurs with sufficient constancy and distinctness to render it unlikely that it is due to instrumental error or manipulation.

It occurs at a point on the tracing subsequent to the time when the pressure in the veins having risen on account of the auricle filling, giving rise to the wave marked *v*, and the pressure having fallen due to the opening of the auriculo-ventricular valves in consequence of the ventricular diastole having begun, the pressure in the auricles again rises on account of the continued inflow of blood into them. It occurs, in fact, when the ventricle is in diastole, and is filling with blood. This raises the presumption that it is due to auricular contraction.

If it were due to the continued passive filling of the ventricle, auricle, and veins, the rise in the wave might be expected to continue to occur until the occurrence of the sudden rise of the curve due to the following ventricular contraction. There is, however, a very definite fall of pressure, as indicated by the descent of the wave, which seems to indicate the relaxation of the auricle after its contraction.

It bears a very constant relation to the preceding ventricular and auricular contractions; moreover, the period from *a* to *a'* is about three-fifths of a second, and that from *a'* to the succeeding *c* is about four-fifths of a second. The former period is very constant; the latter varies somewhat. Now, whether *a* (in the wave *c a*) is due to an auricular contraction from a normally produced stimulus or to a retrograde stimulus from ventricular contraction causing an auricular contraction, which at the same time abolishes the normally produced stimulus material at the sinus, *a'* should follow it after a period a little longer than from *a'* to *c*, because *c* occurs before the normally produced stimulus can produce the auricular contraction causing *a*, unless there is a sinus arrhythmia of alternately long and short periods of a very regular character—that is to say, an auricular bigeminy—the second auricular contraction of the bigeminy not being followed by a ventricular contraction. Another explanation of the bigeminy is that the wave *a'* is due to a heterogenetic auricular contraction or extrasystole, its constant time relationship to the preceding contractions of ventricle and auricle being due to there being some causal connexion between them. The question, then, arises, Is the bigeminy of the auricle due to a sinus arrhythmia, or is it due to auricular extrasystole? The auricular contraction producing *a* in *a c* is due to retrograde stimulation, and is not due to sinus stimulation, and the reason for this is the constant time relation between *a* and *c*. If *a* and *c* were due to stimuli arising quite independently—namely, *c* from an idio-ventricular stimulus formation and *a* from a stimulus arising at the sinus—it is not likely that they would bear such a constant time relationship to one another as they do, over a period of days at least.

If, then, *a* in *c a* is not due to sinus stimulation, it is unlikely that *a'* will be either, and for a like reason—namely, that if it arose from a cause entirely separated from that of *a* in *c a*, it would not be likely to bear such a constant time relation to it over so long a time. On the other hand, if it is due to an extrasystole of the auricle, its constant time relation to *a* in *c a* is reasonable. The possibility of the wave *a'* being due to variations of pressure in the auricle due to passive movements of the ventricle and auriculo-ventricular valves caused by the onflow of blood from the veins, cannot be altogether dismissed, the length of time occupied by ventricular diastole accounting for the fact that the waves due to the variations of pressure are so noticeable. If this is so, the sequence of events is as follows: At *v* the pressure falls due to the opening of the auriculo-ventricular valves, after which there is a sudden rise in pressure in the ventricle causing a closure of the valves producing the short wave occasionally distinctly seen just before *a'*; *a'* is produced by the continuance of the filling of the auricle, and the fall represented by its descending limb is due to a second opening of the valves. On the whole, I think it must be admitted that the wave *a'* is not due to variations of pressure due to passive movements of the ventricle and auriculo-ventricular valves. The pressure needed to open the valves under these conditions must be very slight, and could not be sufficient to cause a wave such as *a'* represents in the venous pulse. The assumption that it is due to an auricular contraction offers an explanation of the constant time relationship which the wave *a'* bears to the previous *c a* wave, if the mechanism of a bigeminal action due to premature or extrasystolic contraction of the auricle be accepted.

The outstanding feature of the polygraphic tracing is the infrequency of the radial pulse.

What is the explanation of this infrequent pulse-rate? The commonest cause of a pulse beating at the rate of 40 to the minute, or thereabouts, is the occurrence of an extrasystole succeeding each normal beat, the ventricular contractions of the extrasystole being too feeble to send the pulse-wave to the wrist, or that the extrasystole arises in the auricle and the passage of the impulse to the ventricle is blocked.

Another explanation to be offered is that the case is one of heart-block, either complete, or in which every other impulse is blocked at the auriculo-ventricular junction. It may be advanced as an explanation that a stimulus normally produced, and giving rise to the wave at *a'*, is so long delayed by impaired conduction that the ventricular contraction resulting from it does not take place until a very short interval of time before the succeeding auricular contraction. If this is so the *a–c* interval is an extraordinarily long one, namely, about $\frac{3}{5}$ to $\frac{4}{5}$ of a second. The normal *a–c* interval is from $\frac{1}{10}$ to $\frac{2}{10}$, and pathologically the longest recorded is about $\frac{3}{5}$ (Lewis). Complete heart-block is unlikely, though by no means impossible, with a pulse frequency of 40 to the minute. It is also unlikely on account of the general condition of the patient, and the fact that there have been no fainting attacks. To suppose that it is due to complete heart-block is to assume that, although the auricle and ventricle are beating independently, the ventricular contraction happens to fall every time at an alternate auricular contraction, which is to assume the occurrence of a too great coincidence. That the case is not one of true bigeminy, as defined by Professor Wenckebach, is obvious from the fact that the effective contraction does not originate at the sinus.

There may, however, be a bigeminal contraction of the auricle which is not followed by a ventricular systole.

The case may be classed as one of nodal bradycardia, being one of those uncommon ones in which the pulse beat is regular. If we assume that the wave *a'* is due to auricular contraction, and in the light of that interpretation we look for an explanation of the tracings taken on December 5th, 10th, and 16th, we see that a wave due to auricular systole, whether resulting from an impulse normally produced at the region of the sinus or in the nature of a bigeminy due to auricular extrasystole, appears in a position intermediate between those due to the ventricular contractions. But because conduction is impaired this fails to produce a ventricular contraction.

The failure of the ventricle to contract can be explained as due partly to the refractory state of the ventricle from its previous contraction, the period between the end of the ventricular systole which has occurred and the beginning of the next *a'* wave being 1½ fifths only. The ventricle is, however, at the time of the arrival of the impulse from the auricle in a state of diastole, and should therefore be in a condition capable of contraction. The fact that it does not occur, therefore, shows some degree of impairment of conduction or excitability of the musculature. The difficulty arises that *a'* is not followed by a wave due to ventricular systole on account of block, but the ventricular systole is able to produce an auricular contraction by retrograde stimulation.

At the same time it is difficult to conceive that the ventricular systole should always bear the same time relationship to auricular systole that it does, over so long a time, unless there is some direct connexion between them. What the connexion is, unless it is retrograde stimulation from ventricle to auricle, it is difficult to say. If there were such pathological changes in the junctional tissues that no stimulus could pass from auricle to ventricle, there would be complete dissociation of auricular and ventricular rhythms, and they would not bear a constant time relation to one another.

Although, however, the power of conduction is so far reduced by organic changes, or by nerve influence, as to prevent *a'* being followed by ventricular systole, on account of the proximity of the previous ventricular contraction, by the time the next ventricular systole occurs it may have sufficiently recovered to permit a retrograde stimulus to cause an auricular systole.

However this may be, this contraction of the auricle, not having been responded to, a spontaneous contraction of the ventricle takes place before a normally produced auricular contraction is due. As a result the wave due to the ventricular occurs before that due to the auricular contraction.

During the period following the previous ventricular contraction stimulus material has been formed in it, and the ventricular muscle has been recovering from its refractive state, the two conditions approaching a state suitable for producing a homogenetic idio-ventricular contraction. The time taken for the process to be completed normally in the ventricle is at least 7½ fifths of a second. In the present case the period between the end of the ventricular contraction and the beginning of the next is about 5½ fifths. The explanation of the fact that the sinus rhythm controls the ventricular in the normal mechanism is that the formation of stimulus material at the sinus takes place at a greater rate in it than it does elsewhere in the heart. If for any reason the idio-ventricular rhythm is not dormant, and the periods of time between the formation of stimuli in it are shorter than in the sinus, the ventricular rhythm will predominate.

In the present case the idio-ventricular rhythm is not dormant, and the rate of stimulus formation in the ventricle being more rapid than is usual, combined with the fact that no effective stimulus has reached the ventricle for so long a time, the ventricle escapes every cardiac cycle, as seen on the tracings of December 5th, 10th, and 16th.

This ventricular systole due to the escape of the ventricle causes a retrograde stimulation of the auricle to contract. The conclusion is irresistible that it is not mere chance or coincidence that the ventricular contraction should take place at about the time of the normally-produced stimulus of the auricle with such great regularity.

For this reason I conclude that a retrograde stimulation of the auricle has occurred. In this way the regularity of the rhythm is maintained and the condition is produced of the ventricular contraction, which is effective in maintaining the circulation, arising in an abnormal situation.

Tracings taken on several occasions subsequently have shown a perfectly regular pulse, though somewhat infrequent, and in all other respects normal both in regard to the venous and radial curves.

It follows, therefore, that if the degree of heart-block which resulted in the second of the bigeminal auricular contractions not being followed by a ventricular reponse was due to organic lesions in the tissues of the heart responsible for the conduction of impulses, those lesions have been removed.

Cases of heart-block have been recorded in which the block has been due to a haemorrhage from traumatism or spontaneous rupture of an atheromatous artery in the neighbourhood of the auriculo-ventricular bundle. It is conceivable that such a lesion could clear up sufficiently to allow the subsequent passage of impulses. The case discussed may have been due to haemorrhage in the bundle caused by such a condition. But this is unlikely in the absence of any history of injury, and in consideration of the patient's age and the absence of evidence of atheromatous arteries elsewhere.

The conclusions arrived at from a study of the polygraphic tracings are that the infrequency of the pulse-rate is due primarily to a relative delay in the rate of stimulus formation at the sinus. This, together with an extrasystolic contraction of the auricle not followed by a ventricular response, causes the period before effective stimulation of the ventricle coming from the normal situation to be so long that the ventricle escapes, and in turn produces auricular contraction by a retrograde stimulation, thus producing the establishment of the idio-ventricular rhythm.

FATAL PURPURA FOLLOWING SCARLET FEVER.

BY

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RELATIVELY rarer than the mild purpura chiefly affecting the lower extremities which occurs after scarlet fever is a malignant form with extensive superficial lesions in various parts of the body. The case here reported belongs to the latter type, but is unique in the writers' experience in that the purpuric areas showed a definite inflammatory element. The inflammation was revealed not only by an undue degree of tenderness, but by marked swelling of the loose tissues, and a certain amount of induration. Early in the attack it seemed likely that the lesions would become gangrenous, but the patient died before any breaking down had occurred; also it was noticed before death that at points on the right leg and left arm the purpura was beginning to retrogress.

The patient was a boy aged 6 years, who was admitted to Plaistow Hospital with scarlet fever. There was a history of measles, chicken-pox, and whooping-cough within the two previous years; as a result, it was stated that he became anaemic, but that otherwise his health was good. There was nothing noteworthy in his family history.

At the time of admission the attack of scarlet fever was declining. The case ran the usual course, without complications, and by the end of the fifth week the patient was up, and seemed in ordinary health save for the persisting anaemia. Iron and cod-liver oil were prescribed. In the seventh week moderate tonsillitis set in. The boy's temperature rose to 100° F. He was kept in bed, and in two days the pyrexia disappeared. The tonsillitis, streptococcic in origin, also rapidly subsided. The patient, however, was not allowed up as the anaemia had slightly

increased. His blood was examined and showed merely a fall in the red cells with slight hyperleucocytosis. No enlargement of the lymphatic glands could be found, and the spleen was not palpable.

At the beginning of the eighth week, when he was about to be allowed up, a purpuric patch, 4 in. square, suddenly appeared on the outer side of the right ankle. It was dark purple, painless, but rather tender. There was no evidence of trauma. In the evening the temperature rose to 100° F., and by the following day the patch was slightly larger. Towards night similar areas developed on the extensor surfaces of both elbows. The boy moved his arms easily although not willingly. During this night and throughout the following day the blotches on the arms spread rapidly, and a fresh one was found over the inner malleolus of the left foot. The boy now became restless, but did not complain of pain except when the affected parts were touched. He refused to move himself, but often asked the nurse to change his position.

On the morning following the next day the left ear was affected. It was deep purple in colour, and very swollen and tense. From the ear the lesion extended within twenty-four hours over the whole of the left side of the face, scalp, and neck. This, the largest area, was sharply margined. During the same period a patch was noticed on the outer side of the left thigh, and in twelve hours had a diameter of 6 in.

On the morning of death the following was the patient's condition: He was markedly anaemic but mentally clear. There was great disfigurement of the face, more particularly in the region of the orbit and mouth, the lesion being here still unilateral. Small black blebs had formed on the ear, the face, and one extremity. The contained fluid proved sterile under ordinary cultural tests. In continuity with the lips the left buccal surface was discoloured. An area about three inches in diameter was present on the extensor surface of the right elbow. Another covered the whole posterior surface of the right hand. The palm of the hand was unaffected except for a purple tingeing of the flexor lines. A fading greenish patch occupied the back of the left elbow. Another was present on the dorsal surface of the hand and first two fingers. An area on the outer side of the right ankle presented the greenish and yellowish tints of a fading bruise. This was the earliest lesion. There was a purpuric spot near the nail of the great toe. A large circular patch, 6 in. in diameter and with a very definite margin, covered the middle of the thigh on its outer side. Over the front and inside of the left foot there was an irregular reddish-purple patch with a few dark vesicles on it; a similar patch was over the patella. The skin on the trunk and abdomen was unaffected.

During the three days before death the temperature varied irregularly between 100° and 103° F., and the pulse steadily weakened. A blood count taken on the day of death gave 3,600,000 red cells and 50,000 leucocytes. A differential count yielded the following percentages: Polymorphonuclears 88 per cent., small lymphocytes 4 per cent., large lymphocytes 6 per cent., eosinophiles 2 per cent. No nucleated red cells or myelocytes were found in the films. At no time during the illness was there haemorrhage from the mucous surfaces. Unfortunately an autopsy could not be obtained.

UNDER the will of the late Miss Hannah Elizabeth Gartside, of Holmfirth, Yorkshire, King Edward's Hospital Fund for London, the Brompton Hospital for Consumption, St. Thomas's Hospital, and Guy's Hospital each receives a sum of £1,000.

WE learn from the *Journal of the American Medical Association* that in a recent case the eighth circuit of the United States Circuit Court of Appeal affirmed the conviction of one Ackley who had sent a letter through the mails recommending a lady's syringe and sanitary powder for the purpose of preventing conception. The statute declares unlawful every article or thing . . . described in manner calculated to lead another to use it for preventing conception. The article might be of legitimate use and might not prevent conception, but would nevertheless be unlawful if calculated to lead another to use it for such purpose. British law is, we believe, silent on this subject.

ACUTE FACIAL DERMATITIS PRODUCED BY A HAIR DYE.

BY

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THE use of dyes for the hair is admittedly common, but there are so few records in medical literature of accidents resulting from their application that the publication of the following illustrative case may serve a useful purpose, both as a guide to diagnosis and possibly as drawing attention to the dangerous nature of the most commonly employed chemical substances in this connexion.

A married lady, aged 48, engaged in a profession in which public platform appearances play an occasional part, came for the treatment of an acute blotchy erythema limited to the face and scalp, and associated with very acute subjective sensations of burning and itching, severe enough to interfere with sleep and prevent completely the carrying on of her daily routine. These symptoms had been present for forty-eight hours, and had gradually increased in severity. There was considerable mental excitement. The severity of the inflammation had produced oedema of the eyelids, but no vesicles or papules were evident. Although she complained of attacks of shivering and felt thoroughly ill, the temperature and pulse were both normal and the tongue only slightly furred. There was a history of three such attacks (each lasting four to five days) in the preceding eighteen months; she had never previously had any skin trouble. Her scalp, however, had been irritable for two years, and the various lotions prescribed for this and for the acute attacks appear to have relieved without permanently curing the condition. She was quite convinced that every attack had had its origin in the scalp. An examination of this revealed only hyperaemia and a few scales, which on microscopic investigation with methylene blue proved to be sterile. The idea of a seborrhoeic basis for her complaint was therefore abandoned. The colour of the hair, a sort of matt gold, known to ladies as *blond cendré*, had not excited the suspicion of artificial tinting at first, but on the following day a cautious inquiry elicited the important admission that a "harmless" vegetable preparation had been in frequent use for the past three years. Two solutions were employed to obtain the desired tint—the first of an opaque dark brown colour, the other clear and smelling strongly of sulphuretted hydrogen. The former, on analysis, was proved to contain small quantities of a silver salt and traces of pyrogallic acid as pyrogallate; the second was a weak solution of sodium sulphate.

It is not my present purpose to discuss the nature or *rationale* of the chemical staining powers of these substances or of their interaction; but it cannot be too strongly emphasized that pyrogallic acid is an irritant to the skin of susceptible individuals. It may also produce a toxic nephritis by absorption, and its local caustic action in strong solution is often made use of for the destruction of nodules in ulcerating lupus. In weaker solutions it may initiate an irritation akin to that produced by chrysarobin, for which it was originally introduced as a substitute. The effects are most commonly seen on the hands of photographers, many of whom employ it or one of its derivatives (hydroquinone, metol, etc.) in the development of negatives. In these patients also the complaint usually is that they have been using the substance for years without any ill effects, and that they cannot understand why the dermatitis should have appeared only recently. The explanation of this fact is not clear, but may depend on the gradual production in the patient by repeated small doses of the noxious drug of the anaphylactic state, in which the very smallest quantities of it, if rubbed into a small area of the patient's skin, will produce in a few hours (twelve to twenty-four) a very marked reaction (Judassohn's test). Hair dyes may, for purposes of classification, be divided into those which produce their effects by simple staining—for example, walnut juice—and those which depend on a chemical action. To the latter and more dangerous class most of the advertised preparations belong, partly because the

general effect obtained is more striking, and partly because it is cleaner and more permanent, requiring renewal about once every three months only, as the hair grows. A member of this class is paraphenylenediamin, which produces a reddish tint and is very poisonous. Many other preparations are in vogue, the compositions of which are trade secrets, and emanate mainly from France. The great majority are chemical combinations of silver, lead, or mercury with pyrogallic and nitric acids. Potassium permanganate and hydrogen peroxide are also used, but are apparently less often the cause of acute inflammation. Their main effect is to bleach the hair.

The treatment of a "hair-dye" dermatitis is usually a simple matter, and a dilute solution of lead or aluminium acetate in water (1 in 100), frequently applied on a mask of lint with the patient reclining, is all that is required in the early stages. Zinc creams with a trace of ichthyol (mx ad ʒx) will be found soothing for the face after two or three days, when exfoliation will have begun, but they are usually not tolerated at first.

Alcohol and stimulants of all kinds are, of course, contraindicated. Aspirin is very useful. It is needless to emphasize the importance of warning the patient never to use the offending substance again, for each succeeding attack is more severe and less amenable to treatment than the last.

ON THE GENESIS OF CANCER.

BY

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Preliminary Communication.

WHY does a piece of tissue transform into a fungoid mass? Such is the problem that has hitherto baffled investigation.

The only fact of any real importance established is that there exists a relation between strain on the one hand and cancer on the other. To give five examples:

Antecedent Factor.	Tissue.	Result.
1. Friction of tooth or pipe...	Tongue or lip	"Epithelioma."
2. Strain of blow	Breast	"Carcinoma."
3. Strain of blow	Bone	"Sarcoma."
4. *X* rays irritation	Skin	"Epithelioma."
5. Strain	Right horns of certain Indian cattle	"Epithelioma."*

* Mentioned by Dr. Bashford at the International Medical Congress, August 8th, 1913. In Cashmere the native wagons are dragged by oxen with the rope attached to the right horn, and it is usually in the right horn that cancer develops.

This simple relation is the only one definitely established up to date, namely:

Irritation or chronic mechanical strain = Cancer.

Or, to give a definite illustration, take the case of a tooth rubbing on the tongue:

Antecedent factor (chronic irritation of tooth) → Internal strain in tissue → Internal change in tissue → Cancer.

Now, in approaching this problem, there are clearly two lines of attack:

1. From the cancer side, the one usually attempted.
2. From the side of strain.

It was decided in the present research to proceed from the standpoint of strain and to investigate the effects of strain upon the normal tissues.

To effect this inquiry three things were necessary:

1. A reliable and impersonal method of estimating strain.
2. A tissue of the body that can be measured and upon which strain falls specially heavy and in excess.
3. A sufficient number of normal tissues to measure.

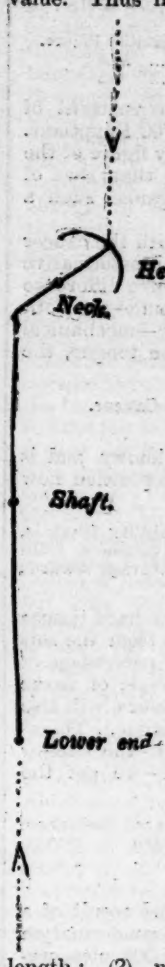
These three points were met as follows:

1. *The Internal Variability of Tissue.*—This can be measured by the usual variability figures. These depend in principle upon the extent of spread of a series of measurements to opposite sides of a mean or average value. Thus if the mean figure of 100 units reaching from 10 to 100 be 55, then obviously the spread of this series of units falling under the measurements thus:

$$\frac{10:20:30:40:50:60:70:80:90:100}{10\ \ 10\ \ 10\ \ 10\ \ 10\ \ 10\ \ 10\ \ 10\ \ 10\ \ 10}$$

would be greater than the spread of this series:

$$\frac{40:50:60:70}{25\ \ 25\ \ 25\ \ 25}$$



The variability figure can be calculated out for each of these series on the principle of finding (1) the average of a series, and (2) the deviation of a series, and dividing the one by the other:

$$\text{Variability figure} = \frac{\text{Total deviation of series.}}{\text{Average of series.}}$$

2. *The only tissues that can be accurately measured are the bones*, for they are fixed with calcium, and are thus measurable with great exactitude. The only bone where strain falls in excess is the thigh bone, for the neck of that bone has to bear not merely the vertical weight of the body, but also the oblique strain as well (see diagram). Every medical man is aware that the neck of the femur is subjected to an excess of strain; for, first, there are special internal strengthening buttresses of bone with special names, and, secondly, the bone yields and breaks readily at the neck in old age.

3. A sufficiency of normal tissues was secured through the kindness of Professor Macalister, in the ancient Egyptian bones lying at Cambridge.

Measurements.—Four measurements were done on each bone: (1) Total length; (2) narrowest at neck (where strain falls heaviest); (3) narrowest at mid-shaft (middle of bone); (4) narrowest antero-posterior measurement at lower end.

The measurements were made by Miss Scrymgeour of North Berwick; the calculations were carried out by Mr. John Dougall, mathematician, Glasgow.*

Results.

The variability figures worked out as follows:

	Length.	Neck.	Mid-shaft.	Lower End.
First 100	5.9	10.1	8.7	9.0
Second 100	3.5	11.3	7.5	7.9
Third 100	4.2	9.3	7.3	8.3
Fourth 100	5.4	9.6	8.2	8.0
Fifth 100	5.1	11.0	7.7	7.6
Sixth 100	7.8	10.2	9.7	8.9
Seventh 100	10.9	13.3	11.8	11.2
Eighth 100	6.6	10.8	9.5	8.6
Ninth 100	5.4	10.2	7.8	8.2
Tenth 100	7.5	12.9	9.7	10.0
Eleventh 100 (85)	5.6	9.0	7.2	8.2
Broken Femora.				
First 100	—	9.3	7.5	—
Second 100	—	10.2	7.7	—
Third 100	—	9.6	7.1	—
Fourth 100	—	10.4	8.1	—
Fifth 100 (80)...	—	9.8	7.8	—

* A detailed report on the research will be published in the *Glasgow Medical Journal.*

Putting the results concisely, we get the following: While the mean figure for the neck is lower than that for the shaft, its variability figure is higher.

	Mean Figure.	Variability Figure.
Neck	2.24	10.4
Shaft	2.28	8.3

These results may be regarded as an experiment of Nature carried out during the lives of some 700 Egyptians. In every hundred of the 1,600 the variability figure of the neck (the point of special strain) is higher than that of other parts of the bone. The chances against such a relation being accidental are as 65,535 to 1.

What relation has such a discovery got with the cancer problem? Simply this, that the reactive transformative changes in cancer in some cases and the reactive difference in the variability figure of this bone—the femur—are both dependent on the identical antecedent factor—mechanical strain. As already stated for the case of the tongue, the sequence in cancer is—

Mechanical strain → Internal strain → Internal change → Cancer.

The third factor—internal change—is unknown and is the heart of the cancer problem. The proposition now established for the hard tissues is—

Mechanical strain → Internal strain → Increased variability (that is, increased divergence from the mean or average towards the extreme).

As there is no reason to suppose that the hard tissues should react to mechanical strain differently from the soft tissues merely because they contain a higher percentage of calcium, it is safe to conclude that any principle of tissue transformation established for the hard tissues will also hold good for the soft—for example, the tongue. Therefore, combining the above two propositions—the one for the soft tissues, the other for the hard—we get the following:

Mechanical strain → Internal strain → Increasing divergence away from the average and towards the extreme → Cancer.

The figures above stated represent the end result of a research into the nature of the internal transformation changes of tissue. They are based upon 25,000 measurements and over 300 calculations. They establish a principle of the first magnitude in the cancer problem—namely:

> There exists a relation between the strain to which a tissue is exposed and the extent to which that tissue varies.

This principle offers a rational explanation of the transformation of normal into cancerous tissue without it being necessary to think of any external cause for cancer whatsoever—for example, parasites. The above results indicate that cancer is explainable as an internal, compensatory, and essentially physiological tissue-change. If the above results be true then cancer is not infectious.

ELECTRARGOL IN SMALL-POX.*

BY

R. DENMAN, M.R.C.S.ENG., L.S.A.,

DIRECTOR, MEDICAL AND HEALTH DEPARTMENT, MAURITIUS.

THE following case is interesting not only for itself, but also for the events it led up to.

A young Mauritian creole was taken ill at the end of our last plague epidemic, early in March, 1913, and was removed to the lazaret for treatment. His case at the commencement was not normal, and twenty-four hours later he developed a rash which turned out to be small-pox. The diagnosis of plague was confirmed microscopically, and it was evident that the lad was suffering from the two diseases simultaneously.

On admission, he was given the routine treatment for plague—namely, intravenous injections of electrargol 10 c.cm. every twenty-four hours—and, as is not uncommon when the patient is treated early enough, the plague symptoms abated. It was noticed at the same time that the rash of the small-pox, which at the commencement looked as though it was going to be confluent, aborted. That on the forehead had already become papular, but got no further. The rash on the rest of the body did not become papular, and the destruction of skin was almost *nil*. The patient recovered, and was dischargod cured within a month.

On talking the matter over with the medical officer in charge (Dr. Keisler) the question as to whether the plague made the small-pox abort or the small-pox the plague, or the electrargol both, was discussed, and we decided that it was worth while to give the electrargol a trial, and as small-pox has been in an epidemic form lately, we have had plenty of opportunities for doing so. We have had over 1,500 cases in the last five months, and we have tried the electrargol treatment on about 150 of them, choosing the confluent and haemorrhagic cases and leaving the milder ones to ordinary methods. The results have been most encouraging: if cases are treated before the full development of the rash, no matter how close the papules may be, they go no further. Those that have already begun to suppurate dry up. Those that have not yet suppurated do not do so, and those that are not yet papular remain simple maculae and fade away. The secondary fever does not appear, and the case takes a favourable turn after the first injection.

Some patients refused this treatment, so that we have been able to compare treated and untreated cases. The comparison has always been in favour of the treatment. The cases submitted to it have always made a more rapid recovery, been more free from complications, and recovered with little or no disfigurement, whereas those that have refused have had prolonged convalescence, frequently severe chest, kidney, or bowel complications, and if they recovered have been badly disfigured.

Of course success could not be expected in every case, and as the most severe were chosen for treatment we have had our failures; but even in the most severe haemorrhagic or septicaemic forms we have occasionally met with success, especially if we have been able to deal with them early enough; but as these, if untreated, rarely live for thirty-six hours after the first onset of the fever, they have to be caught very early for the treatment to be of any use.

Our epidemic is not yet over, owing to the difficulty we have experienced in getting the population revaccinated. We are geographically so situated that until we began making our own lymph we had to wait a whole month before we could get help from outside; but at last we are getting the epidemic under, and I hope in a few weeks to be able to follow this with a full report giving exact figures.

We have latterly increased the dose from 10 c.cm. to 20 c.cm., with marked success, and I recommend this treatment to others who are faced with a similar epidemic, and I give the following advice: Begin treatment as early as possible, give the injection intravenously, and give it at twenty-four-hour intervals for three or four consecutive days. Intravenous injection is difficult in children in any case, and more so when the skin is covered with a confluent small-pox eruption, but the treatment should be attempted in every case, and if intravenous injection is impossible an intramuscular one may be tried, though from our experience they are not very useful.

The virus of this epidemic has been very active; the percentage of confluent and haemorrhagic cases has been high and we have had several fatal cases in second attacks. We have had one second attack semi-confluent within two years of the last, and one patient readmitted to the lazaret twice in the same epidemic, the first attack semi-confluent and the second discrete.

Our death-rate for the whole epidemic has been less than 10 per cent., and for the cases treated—namely, all the cases worse than semi-confluent—about 20 per cent. Of the untreated semi-confluent and worse cases the death-rate has been nearly 50 per cent.

* Electrargol is a liquid preparation of colloidal silver, obtained electrically, as distinguished from the older preparations which were made by chemical reduction. It contains ultra-microscopic particles of metallic Ag in suspension; forming a colloidal solution. It is rendered stable by the addition of other colloids, so that it does not agglomerate on contact with electrolytes. It may be injected into the tissues without risk of local precipitation of metal in an inactive state.—*Year-book of Pharmacy*, 1912.

EIGHTY-FIRST ANNUAL MEETING

OF THE

British Medical Association.

Held in Brighton on July 23rd, 24th, and 25th.

PROCEEDINGS OF SECTIONS.

SECTION OF ELECTRO-THERAPEUTICS.

W. Deane Butcher, M.R.C.S., President.

PRESIDENT'S INTRODUCTORY REMARKS.

The President said that occupying, as electro-therapeutists did, a position at which medicine touched upon physics, it had been thought well in recent years to emphasize that position by asking a distinguished physicist to open the proceedings of the Section. On the present occasion Professor C. G. Barkla would give an address dealing with a subject on which he was the great authority in England—namely, secondary radiations.

ADDRESS ON SECONDARY X-RAY RADIATIONS IN MEDICINE.

By C. G. Barkla, M.A., D.Sc., F.R.S.,
Wheatstone Professor of Physics, University of London, King's College.

(*Abstract.*)

Professor Barkla dealt in turn with the three distinct types of secondary rays excited by the primary *x*-ray beam—namely, scattered *x* rays, fluorescent *x* rays, and corpuscular or electronic rays.

I. All substances when traversed by Roentgen radiation emitted a secondary radiation, which was of the same type and had the same penetrating power as the primary radiation itself. To this secondary radiation he applied the term "scattered." The human body when exposed to Roentgen radiations behaved like a cloud of light smoke under exposure to ordinary light. If *x*-ray tubes were used as lamps each person would appear as a semi-transparent bluish mist, with a well-defined outline. The importance of this scattered radiation was due to the fact that it frequently carried away a considerable portion of the energy of the primary radiation. Thus any *x* radiation which penetrated more than a very thin superficial layer of the body was largely scattered by the organ upon which it fell. The percentage of energy scattered might be anything from 70 upwards. The more penetrating the radiation the greater the percentage of the energy lost by scattering. Owing to this scattering the therapeutic action, if such existed, was enormously diminished, although there was some compensating gain, owing to the radiation scattered from surrounding organs being reabsorbed in the organ treated. These scattered rays had the same properties as the primary ones, and injurious action by continued exposure was possible. The danger of exposure to the scattered Roentgen radiation from heavier substances like metals was much less than from light elements, such as those which constituted the main part of the human body, because the former allowed only a comparatively feeble scattered radiation to escape through the surface.

II. The fluorescent *x* radiation was that which was emitted by substances on being traversed by the *x*-ray beam, but with a penetrating power differing from that of the exciting primary radiation. This was analogous to the fluorescent light emitted by many substances when exposed to white light or any radiation of shorter wave-length than that of the fluorescent radiation itself. Chemical combination did not affect either the quality or the intensity of the radiation emitted by a particular element, and the higher the atomic weight of an element the more penetrating—that is, the shorter the wave-length—was the radiation emitted. The emission of two definite fluorescent radiations by many elements—probably by all—complicated the phenomenon somewhat, but usually only one was sufficiently penetrating to escape through air to any considerable distance. When the human body was exposed to an ordinary beam of Roentgen radiation, the secondary radiation of this type usually played an insignificant part in the processes set up; but the discovery of this type of secondary radiation provided a means of obtaining radiators of definite penetrating power, and of accurately investigating the change in properties, therapeutic and otherwise, with the variation of penetrating power or wave-length. The amount of energy of the primary beam which was transformed into fluorescent radiation never exceeded about 40 per cent., and of this only a fraction escaped from the radiating material itself, if, for instance, it was in the form of a plate. It would, however, be quite an easy matter to obtain in this way a fluorescent *x* radiation of definite penetrating power, and of about one-twentieth of the intensity of the primary beam which produced it. If it were the soft *x* radiation characteristic of copper which it was desired to use, and the anticathode of an *x*-ray tube be made of copper, then not only would that copper emit a very intense secondary radiation characteristic of the copper, but even the directly generated primary radiation would consist largely also of radiation characteristic of copper. Mixed with the copper radiation there was, of course, a heterogeneous and more penetrating radiation, but this could be largely eliminated by absorption. By means of a number of anticathodes of various materials in one *x*-ray tube, and a simple device for moving these, so that any one might be made the anticathode, beams of practically any desired penetrating power might be generated.

III. The corpuscular or electronic radiation emitted by bodies when exposed to *x* rays, like the beta radiation from radium, consisted of negative corpuscles or electrons which were hurled out of certain atoms of the substances traversed by the primary *x* rays, with a velocity of from 40,000 to 100,000 miles per second. All the evidence tended to show that it was to this secondary radiation that must be attributed all observed physical, chemical, and therapeutic actions, with the exception, perhaps, of a slight heating effect in substances which absorbed *x* rays. When the Roentgen radiation passed through a substance for an instant only, there were liberated here and there throughout the body of that substance these rapidly-moving electrons, which wriggled about for an exceedingly short time, losing energy, and leaving behind them a trail of positively and negatively charged particles, called "ions." Thus during the transmission of the primary radiations upon the human body electrons, moving with a velocity half of that of light, were being ejected from atoms here and there, and in their flight were producing ions. The action of the electrons, therefore, was to produce a state of electrification throughout the whole of the body traversed, and it was to this state of ionization that all therapeutic action must be due. The ionization, not only of the tissues of the body, but of all the constituents, was certainly stimulating to chemical action, for this was, of course, fundamentally electrical. Professor Barkla showed photographs to demonstrate, what had previously only been suspected, that all the ionization produced in a substance by *x* radiation was due to the action of the secondary corpuscular radiation, as well as other photographs from which he gathered the inference—photographic effects being taken to correspond with therapeutic—that the therapeutic action was dependent upon the energy of the electrons actually absorbed in the organ treated, or more directly upon the number of ions produced in that organ. The essentials for the production by Roentgen radiation of any measurable effect in an organ traversed by that radiation were, first, a transformation of the energy of Roentgen radiation into energy of corpuscular radiation; and secondly, in addition, the absorption of this corpuscular radiation by the organ to be treated.

Attempts had been made to utilize the action of secondary rays by placing against the body plates or leaves of metal, so that either the Roentgen radiation after passing through a portion of the body fell on these plates, or first passed through the plate or the powdered metal. It should be remembered, however, that a layer of tissue of the

thickness only in the neighbourhood of one hundredth of a millimetre was affected by these soft rays. Similarly, in the immediate neighbourhood of the bones in the body, the flesh was exposed to a strong corpuscular radiation from these bones, and thus underwent a treatment which might be twenty or thirty times as strong as that to which the rest of the body was subject. Where heavy elements occurred in the body, these, or rather the regions immediately surrounding them, were the regions of greatest ionization. The introduction of heavy elements into the body, by injection or otherwise, so that the heavy substance was distributed in a finely divided state within the organ to be treated, meant that this element acted as a transformer, absorbing the penetrating x radiation, and re-emitting the energy in the form of corpuscular radiation which was readily absorbed by the surrounding tissue.

In conclusion Professor Barkla referred to the recently discovered fact that x rays might be partially reflected from the cleavage planes of crystals, exactly as light was reflected from a mirror. By bringing the reflected rays to a focus with a suitably curved crystalline plate, the action over a limited region might be enormously intensified.

DISCUSSION ON RADIUM AND RADIO-THERAPEUTICS, CHIEFLY IN MALIGNANT GROWTHS.

OPENING PAPERS.

I.—Sir ALFRED PEARCE GOULD, K.C.V.O., F.R.C.S.,
Senior Surgeon, and Surgeon in Charge of Cancer Out-patient Department, Middlesex Hospital.

THE USE OF GAMMA RADIATIONS IN SURGERY.

SIR ALFRED PEARCE GOULD, after stating that he proposed to limit himself to a part of the subject of which he had had some personal experience, namely, the influence of the emanations of radium upon malignant growths, proceeded:

Two great facts have emerged from the exact experimental research upon cancer which has been carried on with such great enthusiasm in this and other countries during the past few years. These are, first of all, the enormous power of growth possessed by the cancer cell; and secondly, what is an equally important fact, and one much more cheering to us poor humans, the existence in the tissues of our bodies of a power of resistance to these cancer cells—a power which can hold them in check, and which may be so considerable as to prevent altogether their development. I make no apology, therefore, for prefacing what I have to say this morning by referring to these two fundamental facts.

The power of resistance, or the "immunity reaction," as it has been called by Dr. Bashford, who has done splendid work in connexion with this subject at the Imperial Cancer Research Laboratories, is a thing which does not appeal to any of our senses; we cannot see it, and we are apt, as physicians and surgeons, to ignore it. For many years I have been in the habit of teaching my students at the hospital that this power is the surgeon's great ally. When one thinks of the enormous power of the cancer cell to multiply, and of the great ability of these cells to disseminate themselves, who of us surgeons dare say that any operative successes we obtain in the removal of cancer are entirely due to the wideness of our operations, to the skill with which they are carried out, or to any surgical procedure whatever? I shall never say otherwise than that any case of cancer which seems to have been cured by a surgical operation has really been cured as a result of the combined work of the surgeon and of the great natural power of tissue resistance.

Certain definite scientific knowledge has been obtained about the influence of radium on the cancer cell and on this resistance. First of all, we know that radium—and when I speak of radium I speak of other radio-active bodies as well—has the power of influencing cell multiplication and development. It would take too long if I were to go fully into the evidence in favour of that conclusion. It has been proved experimentally, as you well know. It has been shown that radium emanations can both stimulate and check cell growth. It is to its power of checking cell growth that I especially refer just now.

The second great fact about radium and cancer is this—that the power of radium in checking cell multiplication and growth is greater upon the cells of malignant tumours than upon the normal physiological cells of our body. It is a very difficult thing to estimate precisely that power, but Dr. Russ, who has been working at the Middlesex Hospital Cancer Research Institution, tells me that he has satisfied himself that the power of radium emanations over the life-history of a cancer cell is twenty times as great as it is over the life-history of a leucocyte. But he has obtained certain evidence of this power which carries us even a step further than that. He has found that the influence of radium emanations upon the multiplication of cells is seven-times as great when applied during the actively dividing stage of the cell as when applied during the resting period. This shows that the influence of radium upon the actual life and division of a cell is something very direct, working without the intervention of any other forces. It is not the resultant of a number of co-operating forces; not the result of any link-like action of a succession of processes of metabolism, but is the direct power of the radium upon the nucleus in which the division of the cell starts. No one has yet thrown any light, so far as I know, upon the actual determining factor in the division of the nucleus and the subsequent changes accompanying cell growth. But at any rate scientific men have pushed their researches far enough to show that radium not only checks it and has a great power over it, but that it has this *direct* influence upon the multiplication of the cell, and that its action is much more intense when applied at the moment of division than when applied at the resting moments.

The next point that I wish to refer to is the influence of radium upon the immunity reaction—the resistance of the tissues to the growth and propagation of cancer. Dr. Bashford, in his latest report upon the Imperial Cancer Research Fund, has made the categorical statement that tissue reaction induced by normal tissues and by tumour tissues is—I use his words—"identical in nature." They are not merely similar, but they are actually the same. Now Dr. Russ tells me that he has shown that radium emanations can affect the immunity reaction power of tumour tissue; they can affect it to the extent of destroying it. But this effect of radium emanations is the result of larger dosage and longer influence than are necessary for the demonstration of the cell-inhibiting power. Radium, therefore, can check the growth of a cancer cell without lessening the immunity reaction of the tissue, but if pushed too far it can both kill the cancer cell and at the same time destroy the immunity reaction power of the tumour tissue. That is an observation of most fundamental importance.

It may be of interest to describe the experiments on which Dr. Russ bases that statement. He has taken a thin slice of mouse cancer and exposed it to radium emanations for an hour. On inoculating portions of the tumour thus briefly irradiated into mice he finds the results are negative—the power of self-propagation of the cancer cells has been destroyed by the radium emanations. But if mice after inoculation with portions of this irradiated tumour are inoculated with portions of other mouse cancers, the results are also negative, showing that the power of the cancer tissue to produce an immunity reaction has not been influenced by the radium. He then repeated these experiments, but exposed the slice of mouse cancer to the radium emanations for twenty-four hours. It was now found that not only had the power of growth of the cancer cells been destroyed, but its immunity-producing power was also destroyed. For mice treated with this long-irradiated cancer tissue showed no special immunity when inoculated with other mouse cancers.

Thirdly, I would venture to remind you that while this discussion is on the gamma radiations in surgery, we must not overlook the fact that the alpha, beta, and gamma radiations of radium differ—I was going to say *only* differ—in one great feature—namely, in their power of penetration. The alpha rays have hardly any penetrating power, the beta rays very little, the gamma rays very much. That is the great distinction which underlies our use of this remedy. But while these rays differ in this way, we have

alongside it the fact that the less penetrating rays are more destructive than those of greater penetration.

When we come to apply this in our practice we have to remember that in radium emanations we are dealing with an agent which has the power both of stimulating and of inhibiting cell division, and while it can destroy the power of propagation of the cancer cell, can also destroy other living cells. Happily, that power is exerted later than its power on cancer; living physiological tissues have greater power of resistance than have pathological tissues. Further, radium emanations can influence the immunity reaction of animal tissues if a larger dose is applied than is necessary to inhibit the growth of a cancer cell. It is upon these two facts that we have to base our therapeutic use of the remedy. It is evident that radium is a two-edged therapeutic agent. Granted its power of "curing" cancer, it can also burn the patient. Granted its power of checking the growth of the cancer, it can also destroy the immunity against the cancer which the animal had before. Capable of doing good, it is also capable of doing harm, and evidently we must use this remedy with special precautions.

In the first place, we must bring our radium emanations to act upon the tumour tissue as directly and as immediately as we possibly can. We must put the radium *into* the malignant growth if we are to use it not only to the most advantage in checking the growth of the cancer, but with the greatest safety as regards the patient's other tissues. That is the advantage of radium over *x* rays. When used for a perfectly superficial growth—a growth absolutely on the surface—it is doubtful whether there is any advantage in one over the other. But inasmuch as we cannot easily put potent *x* rays into a deep tissue without any risk whatever of affecting the tissues lying superficially to it, while we can do that with radium, it follows that radium has that certain superiority over *x* rays. But I want to insist that radium should be applied *into* a malignant growth, rather than on a healthy surface over a malignant growth. I have seen successes by that method, but I am convinced of this, that the real value of radium is obtained when it is put into the growth if the growth is not actually on the surface of the body, and not when it is applied to the surface in the hope that its influence will penetrate through the healthy tissues.

The second great practical point is that, radium having this power of checking the cancer cell growth at one stage, and at a later stage injuring the tissue reaction of the patient, we must be careful to use it sufficiently to influence cell growth and not enough to injure the tissue resistance or the immunity reaction of the tissues. We have got to hit the mean between one and the other, and we must be on our guard against using radium too long at a time. We must try to use it so as to get its advantage, but not for so long a time or so powerfully as to suffer the disadvantage of which it is capable. Most workers have had experience of the disastrous effects which are possible, and we know, too, that these effects may be very late in making their appearance. They tell us in Paris of a case of *x*-ray dermatitis which only appeared fourteen years after the use of the *x* rays, and of another case in which the disease appeared after three years. Such statements should make us very cautious in our use of this remedy.

Sir Alfred, in conclusion, went on to describe some of the personal experiences he had had in the use of radium. One case was that of a woman admitted to the Middlesex Hospital cancer ward with a malignant growth of the right tonsil, which interfered very much with her breathing and deglutition. He introduced into it a dose of radium on one occasion only, and the growth entirely disappeared. It was now difficult to say on which side of the fauces the disease had been. Fourteen months ago a young man came to him with a spindle-celled periosteal sarcoma of the lower end of the shaft of the femur. He cut down to it, and put in a tube of radium which he left there for twenty-four hours, and a month later he cut down upon it again, took out another piece of growth, and found that the sarcoma cells were very much degenerated. A second application of radium completed the recovery, and the man was now at work feeling quite well, and there had certainly been no growth, and the swelling was ossified.

He had under his care at the present time a young girl, also with periosteal sarcoma of the lower end of the femur, whose parents refused the ordinary surgical treatment of amputation of the limb. Three weeks ago he introduced some radium in three tubes. The case was still under treatment, but there had been diminution in the circumference of the limb already, and the patient had entirely lost her pain. It was not a case of cure, but he mentioned it as one of the clinical experiences which led him to support the laboratory view that radium emanations had the power of checking the growth of malignant tumours.

A patient came to him several weeks ago at the hospital with a scirrhous carcinoma of the right breast. It was obviously a case unsuitable for operation. He introduced a tube of radium into the lump, and the growth had almost vanished. At the same time he had treated some glands above the clavicle, and he was glad to say that these glands also had disappeared.

Another case was one of squamous epithelioma in the upper jaw with an enlarged gland in the neck. The tumour filled up the nasal fossa on the left side. A threefold introduction of radium had entirely removed this growth. He cut out the gland in the neck, and found it was typical squamous epithelioma.

Then he had a patient whom he saw from time to time, with a large rapidly growing tumour in the right iliac fossa. He had introduced radium into that, and it had got very much smaller. In that case he had experienced one of the drawbacks of radium-therapy. He had used the radium a little in excess, and at three punctures of introduction were small sloughs, unfortunately giving considerable pain to the patient—tough, dry, immobile sloughs, which showed no tendency to separate.

A woman in his cancer ward came in with malignant disease of the ovaries. The abdomen was opened to see if it was possible to remove the growth, which proved to be completely adherent. This tumour was steadily growing, and he thought he would try the influence of radium upon it. He therefore introduced a tube for twenty-four hours. Her pain was better—he did not lay much stress upon that, because whatever one did, there was almost sure to be some response of the kind—and the tumour diminished in size quite appreciably. He repeated the dose, and it had the same happy result, and he repeated it again, but, unfortunately, soon after the third dose of radium, the wound through which he had introduced the second tube of radium ulcerated, and an intestinal fistula formed. The influence of the radium, unhappily, was not only exercised for good upon the ovarian tumour which it had checked, but it had also burnt into the adherent bowel. All further interference was stopped, and ultimately the patient died.

A woman, who had a fibro-sarcoma of the thigh removed at another hospital, came up with an enormous recurrence in the calf of the leg, and refused amputation. She had had two doses with three tubes of radium on each occasion, at about four weeks' interval. The bosses from the surface had all disappeared, and the circumference of the leg had become two inches less.

Another case was one of malignant growth in which the ovaries were removed by two operations, and then there was recurrent growth, which the surgeon attempted to remove at a third operation. The intervention was unsuccessful, and the surgeon said he could do no more. The patient happened to come and see him (Sir Alfred), when it was found that the growth had risen nearly up to the umbilicus, with glands in both groins. Through the kindness of Dr. Pinch at the Radium Institute she was treated with radium plates, and the growth had all absolutely disappeared. There was no trace of any of the tumour, and she had since enjoyed excellent health. It was a splendid tribute to the influence of radium, even when applied by means of radium plates.

My last word (said Sir Alfred) must be this: that we must speak with the greatest caution and reserve of the influence of radium as a therapeutic agent. Our knowledge — our really intimate biological knowledge — of these diseases is so incomplete. We do not know the actual cause of cancer; we do not know the actual conditions upon which its growth really depends. And, on the other hand, we are only at the threshold of our knowledge of the powers of these wonderful radio-active

bodies, of which radium stands supreme. We do know enough to say that it has perils as well as advantages, and in view of that double mist of ignorance that hangs about this subject it behoves every one, while looking forward with hope and working with enthusiasm, which is the great aid to successful work, to speak with the utmost reserve.

II.—ROBERT ABBE, M.D.,

Surgeon, New York Cancer Hospital; Professor of Surgery, New York Post-Graduate Medical School.

DR. ABBE agreed with Sir Alfred Pearce Gould that the subject was full of stumbling-blocks, and that the fundamental principles had not yet been fully grasped any more than the cause of cancer had been unravelled.

We meet with a great many mishaps (Dr. Abbe continued), and these are most instructive. The suffering of the patient and our failure to cure the disease are only a stimulus for us to go on and study our failures. It is from that point of view that I can speak this morning of one fundamental principle, although it differs a little from the line of thought which Sir Alfred has initiated. Some ten years ago I began my work on radium-therapy after twenty-five years of incessant surgical work. Forget for a moment that the knife will cut out and the electric cautery will destroy tumours. In this way the patient is sometimes cured, but the disease is never cured; it is only removed. An agent which will cure the disease will be one which produces a retrograde action upon the malignant overgrowth, while it does not produce an equally potent effect upon the normal tissues surrounding it. Now we come to the consideration of the vital force which actuates the cell. When I began my study of radium I repeated most of the biological experiments, and for years I worked at the problem. The influence of radium on seeds and bulbs is most interesting. If a parcel of seeds are exposed to radium action it will soon be apparent on planting them that they have been effectually influenced, the degree of vital force which is lost after over-exposure depending on the duration and intensity of the irradiation.

I have made experiments in this direction which, I venture to think, are fundamental in their illustration of these problems. I allowed a little series of layers of oats, each layer separated by a quarter of an inch from the next, to have the radium playing upon them for a week, and then planted them carefully in rows corresponding to the distances from which each of the layers had been radiumized, together with two rows which had not been radiumized at all. These were allowed to grow for one month, at the end of which time the average height was about one foot. The two rows which had been nearest to the radium were killed. The third row, which was half an inch away, was devitalized. The fourth to the seventh rows, which had been from an inch and a half to three inches away, were most perfect in growth. These four rows had been stimulated, growing larger, thicker, and richer in colour than the rows which had not been radiumized. In the rows the seeds of which had been at four inches' distance from the radium there was again apparent a devitalizing effect, so that the nineteenth row was the poorest row of all except for the two front rows which had been destroyed. This suggests how the stimulating and depressing action of the radium can be localized. I have also tried the effect of a time and not a distance irradiation, and in this case also the same effect took place. After ten or fourteen hours' irradiation the effect was that of overgrowth, and beyond that there was a depression. If this is true it explains many cases of failure, because we have an aggravation of the growth of the cells instead of a destruction or a depression.

Both a stimulating and a retrograde action being obtainable from radium, certain logical deductions follow from the seed experiment. We may destroy a tumour by immediate contact, and beyond that there may be stimulation. But if one does not produce enough radiation of the gamma type to overcome the stimulating action of the rays of shorter range, all attempts to cure the tumour may be vitiated. If, for example, we have only given enough to carry the stimulating rays to the margin, we may produce a marginal expansion of the tumour. To secure a pure gamma radiation must be our aim.

Speaking of his personal cases, Dr. Abbe said that with gamma radiation the most striking and simple instances of cure were those of warts. There was no papilloma of any part of the body, from the typical papillomas which looked oftentimes liked cancer, which could not be cured by means of gamma rays. The laryngeal papillomas were cured to perfection. He instanced the case of a girl, 17 years of age, who lost her singing voice, and afterwards had aphonia. There were papillomas in one vocal cord, and the case was at first pronounced fibroma. Then in a few weeks came an enormous development of true papilloma, covering largely the vocal cords, with more or less loss of the power of breathing, and, of course, no voice. He tried radium upon this girl's throat, giving her an anaesthetic, and putting 100 mg. between the vocal cords, holding it there for thirty minutes. In two months the voice was normal, in four months she had the true range of scale, and now, years afterwards, her voice was as perfect as ever, and she was able to sing quite well. Another case of the same sort extended over to the adenoids. Here again the voice was lost, and this case also cleared up absolutely under radium treatment. Another patient, an elderly lady, had enormous papillomas in her larynx which had existed for years. So troublesome were they that twice a year she had to undergo an operation in order to be able to breathe. She came to him four years ago, when he applied radium, and one cord cleared up and gave no more trouble. For three years—although previously she had had to come up every six months for operation—she required nothing to be done to her; and although there were still some papillomas left, he had no doubt that if he had put in more radium the condition would have cleared up absolutely. The fact was that they were dealing with an enormous hypertrophic cell growth. It was a living cell, which by its riotous overgrowth produced a destructive tumour. Perfect phonation returned in the cases he had instanced.

Speaking of the burying of radium tubes, Dr. Abbe quoted his first case, nine years ago, of an enormous hypertrophic thyroid, in which he practised the insertion of a radium tube. That case had never shown the slightest tendency to relapse. There was in radium a subtle power to affect hypertrophic tissues of cell development. Cases of cancer behind the ear were very common, and wonderful relief was afforded by laying the radium tube upon the growth and giving from one to three or four hours' exposure. One case, of which he showed recent photographs, was treated six years ago, and the result was a perfectly smooth beautiful scar, illustrating the effect upon the tissues at the corner of the eye. A plastic operation would have involved an extensive excision. In another case of typical carcinoma he cut away all that he could and radiumized the wound for eight hours. The wound closed perfectly.

Radium had the power of curing *x* ray lesions. He showed the photographs of the hands of a gentleman who, for fifteen years, had been a maker of *x*-ray tubes. Both hands were covered with small epitheliomas caused by the rays, and these were cured by radium, which was applied for from fifteen to forty-five minutes on each one. Every one of them dropped off perfectly in one month, leaving the smooth healthy skin. The gamma radiation was spoken of as being the equivalent of the radiation from the *x*-ray tube, but it was not the same gamma radiation. He instanced a case of sarcoma of the lower eyelid, sent to him after some weeks of Roentgen-ray treatment. This yielded to radium within a few weeks, and it was not possible to say on which eyelid it had existed. Combined *x*-ray and radium treatment was promising, since gamma radiation was the factor in both agents, and the one was complementary to the other.

He showed finally a photograph of a case of giant cell sarcoma destroyed nine years ago by means of radium, and a recent photograph which testified to the enduring nature of the cure. The pain encountered in some of these cases should not be regarded in the light of a discouragement. It was due to excessive treatment. If they waited patiently they would find it subside, and then they were ready for the final cure.

III.—Francis Hernaman-Johnson, M.D.,
Darlington.

ON THE INDICATIONS FOR RADIATION BEFORE, DURING, AND AFTER OPERATION FOR CANCER.

While all radio-therapeutists agree as to the value of ray treatment in cancer, they are not altogether at one regarding the time at which it should be applied. Leduc, for example, is a believer in lengthy pre-operative raying, and roundly condemns post-operative applications. In this latter view he is at variance with most *x*-ray workers of note. Knox, Morton, and Skinner—to cite a few instances—are convinced of the value of irradiation subsequent to surgical removal; and the last-mentioned authority (Skinner) is further of opinion that pre-operative exposures are harmful.

The main arguments against pre-operative irradiation are three in number. The first is, that parts which have been exposed to rays do not recover well from surgical trauma. The second—which might be urged with equal propriety against any form of ray treatment in cancer—is that the deeper parts of the growth may be actually stimulated. Thirdly, it is said that, once a diagnosis of malignancy is arrived at, it can never be in the patient's interest to delay operation even for a few days.

The first objection—that tissues do not heal well after irradiation—is, I feel sure, a legacy from the past, when surgeons were sometimes called upon to operate in cases where the integument had been rendered blue and unhealthy by excessive exposure to soft unfiltered rays. As I pointed out at Liverpool last year,[1] the effect of moderate doses of hard rays is to stimulate the activity of normal tissues, while depressing the pseudo-embryonic cells of a malignant growth. A part which has been moderately exposed to *x* rays will recover, if anything, more quickly from an operation than one which has not been so exposed.

As to the alleged risk of exciting deep-lying cancer cells to increased mitosis, with modern technique a dosage inimical to such cells can be made to reach any part of the interior of the body.

Lastly, there is the statement that to delay surgical interference is never justifiable. This is not argument, but dogma, a surgical *ipse dixit* which it is high time to examine on its merits in the light of present-day knowledge. It is true that Leduc has already challenged the doctrine, but in such an extreme manner as to discredit his attack.

Radio-therapy, as at present practised, is no substitute for surgery, and any prolonged course of treatment before operation is certainly to be condemned. Nevertheless, I would venture to suggest that even early cases of cancer—cases, that is, in which the least percentage of recurrences takes place—should undergo a radio-therapeutic preparation.

We know that whenever and wherever the knife is employed there is always a risk of disseminating malignant cells, which subsequently take root and flourish. It is not to be supposed, however, that this colonization is allowed to take place without opposition on the part of the invaded tissues. Many of the would-be settlers are no doubt slain before they are able to propagate their kind. Some, nevertheless, usually escape, but if these specially vigorous cells were subjected to previous radio-therapeutic attack, their chances of post-operative survival would be materially lessened. A cancerous tumour in an early stage presents an opportunity for *x*-ray therapy which is never repeated. The enemy is concentrated in close order, and can be assaulted in force. Every cancer cell which escapes removal will then be in a devitalized condition, and in the presence of tissues whose resisting power has been artificially heightened. It is interesting to note here that mouse cancer which has been exposed to radium is grafted with difficulty or not at all into fresh hosts.

As to the time necessary to accomplish this combined depression of malignant and exaltation of normal tissues, in the absence of clinical data we have only analogy to guide us. The dosage may be taken as that usually necessary to bring about the first definite sign of improvement in an ordinary case of recurrent inoperable growth. Every radiologist will have his own ideas on this subject; personally, I think that eight or nine sittings, spread over about three weeks, may be regarded as a working basis.

So far, I have dealt with tumours seen first in an early stage—cases in which surgeons claim considerable permanent success. I have not ventured to do more than *suggest* pre-operative raying under such circumstances. When, however, we come to deal with patients first seen under less favourable conditions—where there is already considerable pain, physical depression, and perhaps commencing ulceration—I think there is ground not only for suggesting but for strongly urging preparatory radio-therapeutic treatment. Such cases, while not mechanically inoperable, are looked upon by surgeons as, for the most part, ultimately hopeless. The friends are usually told that from six months to a year is the limit of life; delay for preparatory raying can therefore scarcely make matters much worse.

Patients of this type are sometimes themselves told the truth, or are strong-minded enough to drag it from their advisers. Not infrequently they refuse an operation which holds out but little prospect of actual cure, and betake themselves to the radiologist for palliative treatment. The majority improve at first in a wonderful manner—tumours diminish in size, pain vanishes, and the body-weight increases week by week. After about two months they seem to the uninitiated to be on the highway to recovery. Now is the time to operate; and let the incision be not less wide and deep because of the retrogression which has occurred.

At this point we may have trouble with the patient and his friends; they cannot see why the risk of an operation should be incurred when everything is going on so well. It is our duty to insist, for we know that while it is true that in a few cases apparent cures are accomplished by radio-therapy alone, yet, as a general rule, after some months the disease again gets the upper hand. Skill and experience is needed to judge the proper moment for surgical intervention; the radiologist should retire some weeks before the crest of the wave of improvement has been reached. Once this point has been passed operation is worse than useless.

Surgeons are already beginning to recognize that hopelessly inoperable cases may sometimes be rendered operable by radio-therapy. From this it is surely no great step to an admission on their part that, whenever the ultimate prognosis is unfavourable despite operation, the radiologist should be called in to prepare the patient for his ordeal.

How are we to get hold of such patients? Fortunately there are many surgeons willing to work hand in hand with us; but in not a few instances Ephraim is wedded to his idols. There remains the general practitioner. Perhaps we may induce him to call the radiologist into consultation before hurrying off each and every cancer patient to the operating surgeon. By so doing we run the risk of having hard things said about our intelligence, and even our moral rectitude may not go unchallenged. But if we use such skill and learning as we possess with a single eye to the benefit of those who suffer, we may safely disregard abuse.

The subject I have just dealt with bristles with controversy, and I am glad to leave it for less contentious matters. I come now to the employment of *x* rays during actual operation for malignant tumour. We are indebted to Dr. Reginald Morton for the suggestion that a full dose should be delivered into the wound before the flaps are drawn together. Where a powerful *x*-ray plant is available in the theatre this seems to me a most valuable suggestion. I would go further, and recommend that in cases of internal growth where, on opening the abdomen, it is found to be impossible to remove the whole neoplasm, a part should be ablated if this is feasible, and *x* rays applied to the remainder. If the conditions prove to be such that the knife cannot be used at all, then the wound flaps should be held widely apart, and a full dose of rays be delivered into the exposed tumour mass.

There remains to be considered the question of post-operative preventive raying. To this alone a lengthy paper might well be devoted, but time does not permit me to do more than refer briefly to a few important points. Surgeons are ready enough to send cases in which there is almost a certainty of speedy recurrence, but hesitate to submit those which they consider favourable. This attitude is born of a desire to "let well alone." It is common

knowledge that *x* rays have in the past caused skin cancer, and it has been stated that they sometimes stimulate an existing malignant growth. To the layman—using the term in relation to our speciality—all rays are alike; yet it would be as reasonable to compare the effect of an injection of salvarsan in syphilis with that of arsenic taken in small quantities on a period of years, as to judge of the action of hard filtered rays therapeutically applied by reference to the results of prolonged exposure to soft irritating radiations. It should be our business to prove that post-operative raying never does harm, and for this reason we must be careful never to push our treatment beyond the stage of stimulation of normal tissues.

Preventive irradiation should include not only the original tumour area, but also *all organs which experience teaches us are likely to become the seat of metastatic deposits*. Such a wide distribution being necessary, it is manifestly impossible to deliver at any given point a dosage actually fatal to stray cancer cells; some amount of depression of these latter is all that can be looked for. On the other hand, to enhance the resisting power of the body tissues is well within the power of a quite moderate amount of irradiation.

In prophylactic post-operative raying it is not permissible to produce any reaction beyond a slight darkening of the skin. As to the method of spacing the applications, I believe in courses of seven or eight sittings crowded into a fortnight, with at least six weeks' interval between each series. The power of the tissues to respond to *x*-ray stimulation is thus, I think, better conserved than if, for example, continuous weekly *séances* are administered over long periods.

The above recommendations apply only to early cases, where operation of itself can secure many successes. When there is grave reason to expect a speedy recurrence, more vigorous measures are called for. In fact, the boldness of the radiologist should be commensurate with the prognostic fears of the surgeon.

Patients who have been operated on for cancer are not usually sent for preventive treatment till at least a month has elapsed. Where there has been a preparatory course, this delay is of no consequence; otherwise, raying should be commenced as soon as recovery has taken place from the initial surgical shock. During the first month after operation the resistance of the patient is probably lower than normal, and it is then that the foundations of future trouble are laid. I have found that if one proposes to treat a rodent ulcer by scraping and subsequent irradiation, an interval of even ten days will seriously affect the ultimate result.

Immediate treatment after major operations is, of course, practicable only where a movable apparatus is at hand. Great weakness is a contraindication, but the presence of raw granulating surfaces after extensive ablation of the breasts and axillary glands is no bar. On the contrary, an open wound will heal more quickly under ray stimulation.

Before I conclude this paper I should like to repeat that the last thing I have in my mind is to advocate radiotherapy as a substitute for operation. On the other hand, ray treatment is not to be looked upon merely as a means of attaining a sort of prolonged euthanasia. Operators demand, and rightly demand, that cases shall be sent to them at an early stage. We are entitled to no less consideration. The treatment of every case of cancer should be undertaken jointly by a surgeon and a radiologist, between whom the closest harmony and co-operation must exist if the patient is to have his full chance of life.

The implements of surgery are comparatively simple, and have changed little in a generation, whereas the weapons which we employ are undergoing constant modification, and could be made a life-study in themselves. Nevertheless, if we wish to rank as the equals of our surgical colleagues, we must study disease no less than electrical technique, and a treatise on cancer should come as naturally from the pen of the radiologist as from that of the surgeon. Only thus can we enter into our proper heritage in the great world of medicine.

REFERENCE.

[1] BRITISH MEDICAL JOURNAL, August 17th, 1912, p. 377.

IV.—FRANK FOWLER, M.D., M.R.C.S.,

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THE TECHNIQUE OF *X*-RAY TREATMENT OF CANCER.

THAT a certain amount of success has attended the use of *x* rays in the treatment of cancer is now generally admitted by surgeons as well as radiologists, and it is our duty to find out how the results attained so far can be improved upon.

I propose to give reasons for the technique which I employ, which I will describe as fully as time will permit, in order that they may be subjected to adequate criticism.

Why are we generally successful in treating a secondary cutaneous nodule while we fail to benefit cases of deep-seated cancer? No one will have any hesitation in answering that in the latter case the dose of rays is insufficient. I believe that this is the whole truth, and that we should endeavour to increase the effective dose which reaches the tumour: (1) By increasing the skin-focus distance; (2) by cross-fire; (3) by filtration; (4) by the use of very hard rays. Unfortunately each method entails more time, trouble, and expense.

1. The possibility of increasing the distance of the tube from the skin is the chief, perhaps the only, advantage *x* rays possess over radium. The wider angle of radiation is important, as well as the greater uniformity of dose that the deep parts receive compared with the dose at the surface. Improvement in tubes and apparatus having made possible the greater skin-focus distance, I have hopes of being able to treat cancer of the liver if I can keep the tube far enough away. In any case I believe it is impossible to get satisfactory results without the aid of powerful modern apparatus.

I take 9 in. as the minimum skin-focus distance, and for tumours of greater thickness or depth 12 in. (15 in. or more would be better, but this distance entails divided doses).

2. *Multiple direction of radiation* is essential to the successful treatment of a tumour of any appreciable thickness, if the deeper part of it is to receive an effective dose; and also, for the reason that all lymphatic glands in the area should be treated in every case, we should not wait for them to become obviously infected.

When treating a scirrhus of the breast for prophylaxis after operation, the scar or wound, the axilla, and the supraclavicular area should be treated, the direction in each case being at right angles to the other two. In cases where there is already recurrence of the growth I also give a fourth dose over the scapula. Cancer of the rectum I treat from the perineum and from above the pubes, keeping the skin-focus distance as great as possible. Glands in the neck can be rayed from two directions on one side, and, if necessary, from the other side as well. I do not think there will be much difference of opinion about these two points—skin-focus distance and cross-fire. I now come to more contentious matter.

3. *Filtration of the rays* enables us to give a larger dose of the more penetrating rays in proportion to the thickness of the filter, provided that the tube is hard enough; it would be useless to attempt to give a pastille dose through 1 mm. of aluminium with a tube capable of giving a satisfactory skiagram of a hip-joint. I do not think it matters what material is used for the filter, *if* the dose is measured *on the skin* by Bordier's or Hampson's pastilles. The skin is certainly the most desirable place for the dose to be measured, but the difficulty in reading small differences of tint has, so far, impelled me to continue using Sabouraud's pastilles, placed at half skin-focus distance, and in this case a metal filter must be used. The pastille is placed not nearer than 2 cm. to the wall of the tube, and the filter between the tube and the pastille; I use aluminium of ½ mm. and 1 mm., increasing the thickness if the treatment is prolonged. The trouble of accurately measuring the half skin-focus distance is the only real objection to this method. I have heard it stated that the heat from the tube deepens the colour of the pastille, but the filter cuts off the heat of the glass wall, and I do not find the metal gets appreciably warm. Another objection was that secondary rays from aluminium would affect the barium platino-cyanide pastille, which I believe to be a physical impossibility, especially at 1 cm. distance.

It is undesirable to put any metallic filter on the skin of the patient, as the secondary rays may be dangerous; the same objection applies to solutions of tungstate. Boiler felt and leather are free from these objections; only, I repeat, if these are used the pastille must be placed on the skin; neglect of this point seems to me to be responsible for the statement that it is impossible to cause a dermatitis if sufficient thickness of filter is used; the colossal doses occasionally reported only reach the skin in infinitesimal amounts when they are given with a medium tube through thick filters. I believe it is a fact that these large doses have all been measured before filtration.

4. It has frequently been stated that a substance is only affected by rays of such quality as it is able to absorb. I believe this statement is incorrect, and that we have been to some extent misled by physicists, who told us that theoretically rays should be as soft as possible. Perhaps the explanation of the difference we find in practice is that hard rays are necessary to produce in the tissues secondary rays of the correct quality to affect the growth. That very hard rays do affect the tissues is shown by their effect on cancer and myoma, by the epilation that follows their use, and by the dermatitis that a sufficient dose will certainly produce, nor do I confine their use to thick or deeply-seated tumours. I find them efficient in some diseases of the skin, such as psoriasis; here the amount absorbed must be very small.

Since writing this I have read a paper by Dr. Hernaman-Johnson, in which he very ably advocates the use of hard rays.

Dosage.

I give 6 or 8 Holzknecht units in each direction once a fortnight for treatment, and once in three weeks for prophylaxis, the dose being measured after filtration. This soon causes a deep brown pigmentation of the skin, but seldom any redness; only when the skin-focus distance is made too great to get a full dose in reasonable time do I give fractional doses at shorter intervals.

Tubes.

For private cases, where I can allow half an hour for each patient, I use Cyclops tubes, and find that, with careful seasoning, I can get them to stand 3 to 4 milliampères without overheating. With ½ mm. aluminium this current gives 6 Holzknecht filtered rays in five to six minutes. With 1 mm. aluminium the same dose takes ten to fifteen minutes. For hospital work I use water-cooled tubes on a Snook; after long use, when they have become deeply purple, they will stand 4 to 5 milliampères. We have given 20 pastille doses in one afternoon with the same tube. I only soften a tube when the current begins to spark round it. An alternative spark-gap of 20 cm. is about the limit of hardness it is convenient to use with our present tubes. To get 4 milliampères through such a tube strains the insulation of the coil and condenser. I run the break as fast as the tube will allow; reducing the speed will harden even a warm tube.

Duration of Treatment.

Where there is a recurrence to be treated, the duration is indefinite. I have some few cases that have been able to leave off treatment and remain free from disease. My partner, Dr. Saberton, and I have attempted to map out a route for prophylaxis, giving what we consider the minimum, which is 4 pastille doses, in each possible direction, in three months; then a rest for two months, and 2 pastille doses in each direction in the next six weeks; six months' interval, and repeat two doses. I cannot say whether this will prove to be enough; a few cases have had recurrences in spite of early prophylactic treatment; but I am confident that the trouble and expense will be warranted by the results, which will take many years to prove.

Results.

We cannot usefully compare technique without mentioning results, so far as can be done at present. *X* rays have already done much to lessen the horror a patient associates with the diagnosis of cancer. I tell a patient, when I begin treatment, that although I cannot undertake to cure the disease, I *can* promise that there will be no ulceration and no pain. The relief that such an assurance gives to the patient helps to prevent the downward course that often follows the diagnosis of cancer; I do not attempt to discriminate between the good that is done by *x* rays and that done by my confidence in their beneficial effect.

There are some disappointing cases which seem to be entirely unaffected by treatment, but these are not met with as frequently as they were a few years ago; I do not know of a single case that I have thoroughly treated where there is any extensive ulceration. Most of the deaths from cancer lately have been from secondary growth in the liver; a few in the bones of the pelvis or vertebrae; in these it is difficult even to alleviate the pain. Many of my early cases died from mediastinal growths, but these have become rare since I have systematically rayed the chest from three or four directions; I consider the mediastinum to be within the range of control. Enlarged glands do not cause me any anxiety; I believe we are justified in claiming that they are better treated by *x* rays than by excision.

I have under treatment several cases who have refused operation, in which the primary growth remains quiescent, but I do not advise *x*-ray treatment as a substitute for operation. In one case of scirrhus of the breast the surgeon removed the growth, and left the axillary glands, which were not obviously infected, at my request. So far we have no cause to regret the omission.

DISCUSSION.

Dr. William Hill (St. Mary's Hospital) described a case of cancer of the oesophagus which was held in check by radium for nearly three and a half years. The patient, a woman, was sent to him in February, 1910, suffering from dysphagia, with marked debility and emaciation. Under examination with the oesophagoscope under cocaine a well-marked stricture with inflamed and ulcerating mucosa, readily bleeding when swabbed, was seen 29 cm. from the upper alveolar margin. It had all the appearances of a malignant growth. The stricture was dilated up by graduated bougieing through the oesophagoscope, and he inserted his intubation apparatus, through which milk and brandy were immediately administered. In the course of a few days the patient could swallow fluids easily by the side of the tube, and this was removed at the end of a fortnight, when it was found that soft food passed readily. Two months later swallowing became difficult again, and *x*-ray examination showed an irregular outline at the mouth of the stricture, suggestive of fungation. An examination was made under a general anaesthetic to make sure of the diagnosis, and a small portion of the sprouting growth was removed for microscopical purposes. This was reported to be an endothelial sarcoma. The intubation apparatus was reinserted to dilate up the stricture, and later, with the co-operation of Dr. Finzi, a tube containing 100 mg. of radium, enclosed in a tube of lead 2 mm. in thickness was inserted in the stricture through the oesophagoscope, and allowed to remain *in situ* for twelve hours. There was remarkable objective and subjective improvement, in fact, all traces of fungation and ulceration disappeared, and the swallowing was almost normal for well-masticated food. Six months later there was a return of dysphagia, but on endoscopic examination there were no objective signs of malignancy. Obvious contraction had taken place, however, necessitating dilating up of the stricture by endoscopic bougieing, the latter procedure not inducing bleeding as formerly. Ulceration and other signs of lighting up again in the hypertrophied growth had appeared on three subsequent occasions, on each of which the radium treatment had been repeated with the same temporary result. The patient now weighed nearly a stone heavier than she did when the treatment was started. The patient was shown before the Medical Society of London in February, 1911. Of 21 cases of cancer of the oesophagus treated by radium salts reported on that occasion, all were dead except this woman. One man, however, who had died from haemorrhage only just previously, had lived for three years after only one application of radium, and his improvement until a little time before had been so great that he had declined further treatment. He returned, however, with the disease too far advanced to justify its repetition, as the trachea showed signs of invasion. Out of 40 cases treated up to the present, there had been very marked improvement in one-third, and some improvement

in others. Dr. Hill handed round *x*-ray prints of the cases before and during treatment, and also showed cases with the radium tubes *in situ* in the gullet. Various forms of screens used by him in cancer of the deep pharynx and of the gullet were exhibited.

Dr. F. H. WILLIAMS (Boston, U.S.A.) had been using radium for twelve years, but during the first two years it was not possible to obtain the pure radium bromide, so that his actual experience only covered a period of ten years. Ordinarily he had used at the Boston City Hospital a capsule containing 50 mg. The work there had been confined chiefly to small superficial lesions of the skin. It was hardly worth while for him to attempt to say much about ten years' experience in a few moments, but there were one or two matters of paramount importance. In the first place, he thought that any interference only aggravated the situation, and that it was better that there should have been no previous treatment, such as the use of caustics. Radium did not do as well as usual if it followed many other kinds of treatment. There was another point worth mentioning, but one about which he was not quite so sure. It was that in some of the more active cases the radium worked more promptly; the results were more readily seen. Radium was the best agent available for keloids. With the amounts he used, all the small keloids had yielded. The larger ones had not done so, perhaps because radium was used in insufficient quantities, therefore the keloids should be attacked when they were small, or at any rate at as early a stage as possible. The results, as a rule, were better with radium than with *x* rays. One advantage of radium was its greater safety. It had a uniform output, and the worker knew what he was doing. This he certainly did not know in the case of the *x*-ray tube. With the pastilles it was possible to measure quantitatively more or less the amount of rays coming from the tube, but he did not think that they could make qualitative determinations easily in that way. By using a small instrument which was something like a comparison photometer, and with radium as a standard of light, the interposition of plates of different thicknesses might enable one to analyse the light which came from the tube. In conclusion, he had something to say about protection from *x* rays. It was easy to make up a material which was a non-conductor of electricity, and quite opaque to the rays, as well as capable of being used in any desired form. For this purpose he employed white lead, worked up into a kind of putty, and this, supported on a wooden holder, enabled one to have quite a thick opaque protection from *x* rays, which, he thought, was better than anything else. Lead glass was not a sufficient protection for the physician.

The President (Dr. DEANE BUTCHER) said that it was important to mention one little point which occurred to him, namely, the danger of taking a specimen for diagnosis. The great advantage of *x*-ray treatment was that it did not necessitate the opening up of the lymph spaces, and he wished very strongly to emphasize this matter. He recalled a painful case in which a sarcoma of the thumb was doing admirably under *x* rays, but in order to make the diagnosis absolutely sure a specimen was taken, and the case forthwith went from bad to worse. He confessed to a sort of shudder on seeing cases go to the hospital and, for the purpose of completing diagnosis and making it clean and clear, having the lymph spaces opened up to further contagion. Whatever they did their first duty was to the patient. The surgeon, on the other hand, was perfectly right; he took a specimen and then he amputated the limb or cut away the part, but the radiologist had no right to take such a specimen, in his opinion. At the same time, the burying of the radium tube within the tissue was a very different thing from cutting out a piece of tissue for diagnostic purposes.

Mr. J. HALL-EDWARDS (Birmingham) said that Sir Alfred Pearce Gould had based his arguments on certain experiments made in the laboratory, and he had described two very wonderful experiments, in one of which a thin slice of cancer taken from a mouse was grafted upon another mouse, which became immune, while with a second section, which had been subjected to radium emanations for twenty-four hours, the mouse was no longer immune. When they came to follow up the treatment which he advised, however, they must get a very clear case. The radium emanations were dying off from a central spot, so that he took it that some of the tissues lying away from the centre of the spot would have an exposure equal to one hour, while others lying near to the filter would have an exposure equal to twenty-four hours. The result certainly must be a mixed one. In view of the danger of opening up the lymph spaces, he thought that where radium could be inserted without making any artificial opening, as, for example, in the uterus or the rectum, it demonstrated its superiority, but in other cases *x* rays were preferable. Dr. Hernaman-Johnson's paper was one which certainly gave them much food for thought. The speaker had had no experience of giving radiations before operation, but he had treated a great number of patients immediately after operation, and taking the cases all round—although it must be many years before a definite conclusion could be arrived at on this point—out of about 60 cases he had treated, he had only heard of about 2 cases in which the secondary growths appeared under twelve months, and in most of the cases the results had been entirely satisfactory. There could not be the slightest doubt that irradiation immediately after operation delayed recurrence if recurrence was likely to follow quickly. In recurrent carcinomas of the breast the thickest filter and the hardest tube would produce the best results. Rays of the same quality as were ordinarily given in the treatment of myomas should be used, and it would be found that the results were better than by using soft rays.

Dr. A. A. WARDEN (Paris), speaking of Dr. Russ's experiments, hoped that his conclusion that even a slight overdose of radium necessarily destroyed tissue-reaction immunity, need not be accepted in its entirety. It was at present impossible absolutely to fix the necessary dose—time and amount—and such a conclusion would indeed disarm the operator. In some cases the actual introduction of radium tubes into a malignant tumour was impossible, and a good result might be obtained by placing the tube buried in the connective tissue of the part as close as possible to the neoplasm.

Dr. JAMES METCALFE (London) could call to mind two or three cases of malignant disease of the breast which had benefited by post-operative radiation. He thought of one specific case at the present moment in which a large mass the size of two fists and much ulcerated was removed by the surgeon, and he gave post-operative applications of *x* rays for some months afterwards at intervals. Although an unlikely case for any treatment, the woman was now absolutely in good health, and he had no doubt whatever that she would continue so. He was thinking also of a case of malignant disease of the rectum in an old man of 77 years, who was seen by several surgeons. Colotomy was recommended, but he applied *x* rays through a speculum to the part, and the patient got quite well, passing his motions freely afterwards, and living comfortably for three years. When it was possible to do things like that in malignant disease, even though the condition were not cured, the *x*-ray treatment amply justified itself. Dr. Williams had said that radium acted more scientifically, but it seemed to the speaker, on the other hand, that radium must necessarily be more erratic. The material in the case of radium could not really be controlled. The worker did not know exactly at any moment the power and capacity of the material he was applying. Radium remained to a large extent empirical, while *x* rays, when filtration was taken into account, had reached such a point of precision that the result obtained could exactly be foretold. For the present, therefore, radium must be regarded as the inferior method of treatment from the strictly scientific point of view.

Dr. C. F. BAILEY (Brighton) quite agreed with the last speaker that there was much more hope of working in a scientific manner with *x* rays than with radium. Some of the statements made that morning by Sir A. P. Gould called for question. Sir Alfred had said that they all knew that the less penetrating rays were much more destructive than the more penetrating ones. But was this

a generally accepted fact? They knew that radium gave out a large number of soft rays, and quantitatively these might be more destructive, but, ray for ray, was it an absolute fact that the soft ray when it set up its ionization possessed an intrinsically more destructive action than was exercised by the hard ray eventually? After pointing out that some cases mentioned by Sir Alfred were of the kind with which radium workers were quite familiar, he referred to Professor Barkla's suggestion that in the future a homogeneous ray, having a definite and measurable amount of penetration, might be discovered. He felt that it was along these lines that *x*-ray work would become increasingly scientific. In the case of radium they were limited to the gamma rays, which represented a very small proportion of the whole sheaf of rays sent out, but he thought the great hope lay in being able to arrive at different orders of penetration. Barkla had suggested that the targets in the *x*-ray tube should be made of a certain metal, and then the preponderating element in the beam would be a particular ray given out by that metal. The other rays could be absorbed away, and a simple ray of one penetration could be found to work with. A good deal had been said in the course of the discussion with regard to screens. Dr. Ironside Bruce favoured thicknesses of felt, and Dr. Reginald Morton and himself preferred tungstate lint, with a rather thin aluminium filter. He felt sure that this was a perfectly satisfactory type of filter for preventing dermatitis. He instanced the case of a patient who had had the breast removed, and who had had no dermatitis, but only a slight bronzing of the skin. She had absolute freedom from recurrence, and was in quite good health. Therefore he thought that there must be a great deal in what Dr. Morton had said in praise of the tungstate screen.

Dr. G. E. Bowker (Bath) referred to the interesting address delivered before the Section at Liverpool last year by Professor Rutherford, who had laid it down as a fact of the greatest importance that by every test he could employ the physical action of the gamma rays of radium was the same in kind as the *x* radiation. The first thing they had learnt in the Section on the present occasion in Brighton was that the universal opinion favoured a hard tube. Another point emphasized in the proceedings had been the necessity of efficient filtration to protect those structures which were not parasitic. They were aware that the more penetrating rays—those which in the *x*-ray sheaf approximated most closely to the gamma rays of radium—were the least harmful in their action upon the physiological functions of the normal cells of the body. Therefore, in these more penetrating rays they had the advantage of a very powerful action directed upon the parasitic organism in its development or in its rapid cell growth, and a less harmful action on the physiological processes of the tissues approximate to the pathological growth. The subject of the immunity of the surrounding tissues, to which reference was made by Sir A. P. Gould, was more or less a new one in connexion with this matter, and all that the opener had said with regard to the transplantation of cancer cells which had been removed from the mouse and kept in artificial surroundings for several hours was of too experimental a nature for any great reliance to be placed upon it at present, particularly when considering the action of radium upon malignant growth.

Dr. Abbe, in a brief reply, said that in his opening remarks he had had no occasion to make any comparison between radium rays and Roentgen rays. He had in his cabinet plastic casts of breast cancers, made at intervals of six weeks, showing how a grave form of malignancy disappeared almost entirely and progressively under Roentgen-ray treatment. And, while he felt the very great value of the Roentgen rays, he admitted also that radium was erratic. It had, as he had tried to show, an element of danger; but this arose out of technique, and was due to the fact that stimulating rays had not been eliminated. The scientific accuracy which speakers and readers of papers at that meeting had brought to the subject of Roentgen-ray work would mark a striking advance in the surgical treatment of cancer, but the small contribution he had been able to make was to endeavour to show that radium also had its merits and its special applications. In cancer of the uterus its value had been demonstrated. In papilloma of the larynx no external application would be of avail, but the use of 100 mg. of radium for thirty minutes would cure any such condition. With regard to the filtering out of the irritating or stimulating rays, he attached great importance to the method of distance filtration through air. With a distance of 2½ in. separating the radium from the skin, so that the tissues were beyond the range of everything save the gamma radiation, they could do in two hours what would occupy them for forty hours by the method of filtering through 2 mm. of lead. In a case of sarcoma of the scalp, about the malignancy of which there was clinically no question, he placed 100 mg. of radium in the bottom of a pasteboard box, stuffed it with cotton, and applied it to one hummock for an hour, and then moved it on to another, giving a seven hours' application in all. The man had to go 500 miles to his home, and was told to report within a fortnight. He came back on the twelfth day, when there was not a trace of the disease. His scalp was as white and normal as any bald scalp he had ever seen. That demonstrated to his mind the utility of distance filtration instead of lead filtration to eliminate the irritating and burning rays. This made short applications possible instead of the long applications which were necessary in order to get efficient rays through lead. With regard to the destruction of fibroids by either method, his suspicion was that they brought about what was done in surgery by removal of the ovaries. The ovaries were being radiumized by hard rays. He thought that the burying of radium in tumours was of great value in certain cases, and he used the method when it seemed proper to approach the centre of the tumour. He had had some fifteen thyroid gland cases, and he never hesitated to make a cocaine cut and to leave in his tube as long as necessary. There was no disaster, because a lymph exudate set up a blockade of the lymph spaces. He added that he did not wish to exploit radium at the expense of *x* rays. There were practical differences between *x* rays and the gamma radiations of radium, and the agents might be considered simply as adjuncts the one of the other.

A NOTE ON CARDIOSPASM.

By Alfred C. Jordan, B.A., M.D.Cantab., M.R.C.P.,
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[With Special Plate.]

Cardiospasm, or spasmodic constriction of the cardiac orifice of the oesophagus, has not long been recognized, although it is not rare when looked for. Hundreds of cases have been recorded, notably by Plummer. The essential features are: (1) A long history, extending over fifteen or even thirty years; (2) difficulty in swallowing, or else regurgitation of food and mucus after eating—this may be constant or may intermit partially or entirely; (3) a corresponding degree of malnutrition, not so great or so progressive as in the case of organic stenosis.

Diagnosis.

X-ray examinations with bismuth emulsion are of the greatest service in the diagnosis. The patient is examined vertically. Sometimes the oesophagus, greatly dilated, is shown full of mucus and food before any bismuth has been swallowed, the contents of the oesophagus being opaque relatively to the clear pulmonary areas (Fig. 1). When the patient swallows an emulsion containing bismuth carbonate, this rests at first upon the horizontal upper surface of the oesophageal contents; the heavy bismuth then falls through to the cardiac orifice and gradually collects in the lower portion of the oesophagus. Typically the dilatation is very great; the oesophagus curves forward in its lower portion, and finally turns downward, ending in a conical point, through which a trace of bismuth trickles in some cases, while in others not even a trace is seen to pass through, and no bismuth appears in the stomach (Fig. 2). I have seen nearly the whole of a bismuth meal (emulsion) still in the oesophagus six days after I watched the patient swallow it. This was in the case of a man aged 64, the trouble having existed more than thirty years. In such cases the patient

is often thought, erroneously, to be the subject of a dilated stomach, and I have known patients "washed out" regularly for weeks; but on observing, with the fluorescent screen, the passage of the stomach tube, it was seen that the tube, if a soft one, doubled back, while a rigid tube pushed the lower oesophageal wall before it at the place where the oesophagus turned forward.

Treatment.

The treatment of the condition is typically American. The problem is to get a bougie or dilator through the cardiac orifice. A reel of ligature silk is used; about a yard of silk is unwound, and the patient begins to swallow the silk. For the purpose of observation with the *x* rays a leaden shot is attached to the end of the silk before the patient swallows it. With the fluorescent screen the shot is observed after an hour or two in the conical lower end of the oesophagus. Eventually the spasm yields for a moment and the leaden shot slips through, taking the silk thread with it. This may occur within a few hours, but it may be deferred for days. The silk, having entered the stomach, soon passes through the pylorus, and begins to travel through the intestines. The patient carries the reel in his waistcoat pocket, the silk, projecting from the corner of the mouth, being passed over one ear. It remains throughout the treatment, and causes little inconvenience. When the whole reelful of silk has been swallowed a second reel is started, the two being knotted together, and when the silk appears at the anus it is cut short as often as necessary. The object of the thread is to furnish a guide; it is held taut while a special bougie (Fig. 3) or catheter with a perforated metal "acorn" (Fig. 4) at its lower end is threaded over it and passed down. To the lower portion of the catheter, above the acorn, a rubber dilating bag is attached (Fig. 5). When the bag is seen (on fluorescent screen observation) to be in the stomach a rubber tube is attached to the upper end of the catheter, and the bag is filled with air through this tube (Fig. 6). The dilated bag is now withdrawn slowly; being wedge-shaped it forms an efficient dilator. These special bougies and dilators are made by Messrs. Down Bros. from descriptions supplied by Sir Arbuthnot Lane. They are exhibited, together with some skiagraphic prints, in the Pathological Museum.

DESCRIPTION OF SPECIAL PLATE.

Fig. 1.—Chest of a woman, aged 39, taken from the front with the patient vertical, showing the oesophagus, greatly dilated and full of mucus and food, forming a large shadow in the upper part of the right side. (Her oesophagus and stomach are also shown in Figs. 10 and 11.)

Fig. 2.—Oesophagus, greatly dilated, taken vertically in the right anterior oblique view in a typical case of cardiospasm in a woman aged 39. A, Aortic arch; the arrows indicate the spasmodic constriction of the lower end of the oesophagus. (See also Figs. 6, 7, 8, and 9.)

Fig. 6.—Dilating bag (Fig. 5) shown *in situ* in the stomach; the bag is shown distended with air prior to its withdrawal. R.D., L.D., Right and left diaphragm; *Cr.*, crest of left ilium; U., umbilicus, marked by a penny. (In the subject of Figs. 2, 7, 8, and 9.)

Fig. 7.—Taken vertically, showing the lower end of the dilated oesophagus, the constricted lower orifice, and a thin stream of bismuth trickling through the stomach down to the great curvature. U, Umbilicus; the arrows indicate the spasmodic constriction of the lower orifice of the oesophagus. Right anterior oblique view.

Fig. 8.—The pylorus and duodenum in the subject of Figs. 2, 6, 7, and 9, taken on the couch after a bismuth meal. The duodenum afforded well-marked evidence of intestinal stasis, for the bismuth returned time after time from the third to the second part of the duodenum, only small quantities entering the jejunum in spite of strong duodenal contractions. *Py*, pylorus; *a*, *b*, *c*, first, second, and third parts of the duodenum; U, umbilicus.

Fig. 9.—Taken on the couch thirty-four hours after the same bismuth meal, showing great dropping of the large intestine. Hardly any bismuth has advanced beyond the middle of the transverse colon. U, umbilicus; *Trans.*, transverse colon.

Fig. 10.—Anterior view of the oesophagus and stomach taken vertically in the subject of Fig. 1, showing the lower portion of the dilated oesophagus, the spasmodic narrowing of its lower orifice (marked by arrows), and the stomach divided in the middle by a long hour-glass constriction proved at the subsequent operation to be due to a cicatrizing ulcer of the lesser curvature. C, P, cardiac and pyloric portions of the stomach; U, umbilicus.

Fig. 11 shows the appearance of the oesophagus and stomach in the same subject as seen on the couch immediately after Fig. 10.

Result of Treatment.

The improvement after dilatation is immediate, the patient being able to take solid food; the gain in nutrition corresponds. Usually it is necessary to repeat the dilatation several times at intervals of a week or a fortnight, and then at longer intervals, before a definite "cure" can be claimed. A relapse may occur after six months or more; it yields to treatment more readily than the original attack.

The case illustrated (by photographs and lantern slides; Figs. 2, 6, 7, 8, and 9) is that of a woman, aged 39, with sixteen years' history of difficulty in swallowing and of malnutrition and constipation. She gained 3 st. in weight within three months of the treatment by dilatation; by this time she had received (but declined) three offers of marriage!

Etiology.

What is the cause of cardiospasm? Is it a disease *per se* or merely a symptom? Experience of spasm in other parts of the alimentary tract, notably the stomach, leads one to suspect the latter. Gastric ulcer is accompanied by extreme spasm, but there is no evidence that ulceration of the oesophagus has any part in the production of cardiospasm. There is no need to postulate ulceration, for persistent spasm may exist (especially at the pylorus) although the region appears healthy at operation.[1] From the analogy of gastric and duodenal diseases, the most fruitful means of attacking the problem of the ultimate cause of cardiospasm should be by making a complete investigation of the alimentary tract in all cases. An initial difficulty occurs from the circumstance that in severe cases the bismuth remains in the oesophagus so long (for example, six days) that no view is obtainable of the stomach or intestines. After treatment, however, this difficulty is no longer present. A complete investigation now reveals, with great constancy, the distended "writhing" duodenum (Fig. 8), the delay in the lower ileum and in the large intestine (Fig. 9) typical of chronic intestinal stasis. My cases are not yet sufficiently numerous to permit me to say this is the invariable sequence. It is well illustrated by the case of the woman already quoted. The fact that great improvement follows dilatation of the cardiac orifice does not prove the spasm to be the primary disease; analogous improvement occurs

Fig. 3.—Bougies with metallic "acorn," the descent of which through the oesophagus into the stomach is observed with the fluorescent screen.

Fig. 4.—Metallic "acorn," showing perforation for the silk thread, which is used as a guide (see text).

Fig. 5.—Catheter with rubber dilating bag and perforated metallic "acorn" used in the treatment of cardiospasm.

DR. ALFRED C. JORDAN: A NOTE ON CARDIOSPASM.

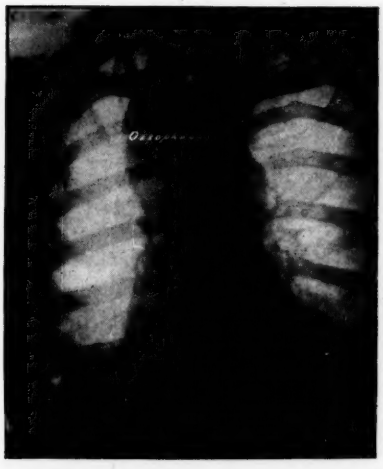

Fig. 1.

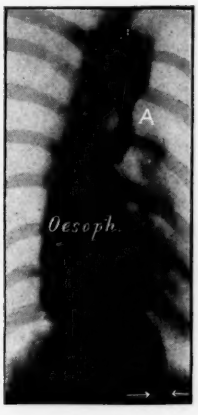

Fig. 2

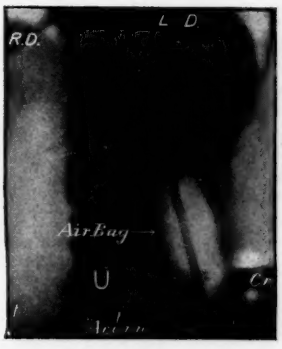

Fig. 6.

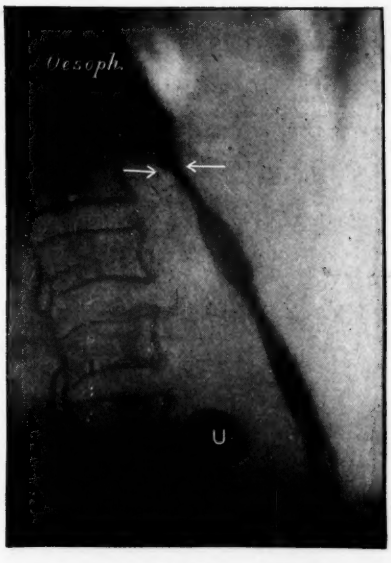

Fig. 7.

Fig. 8.

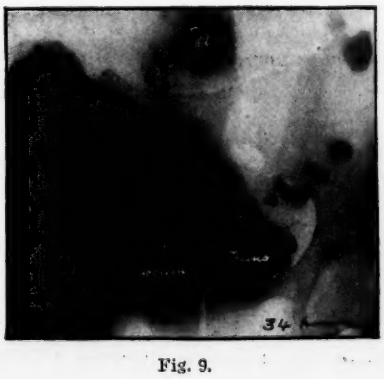

Fig. 9.

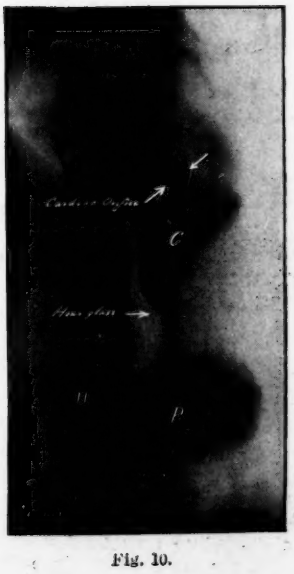

Fig. 10.

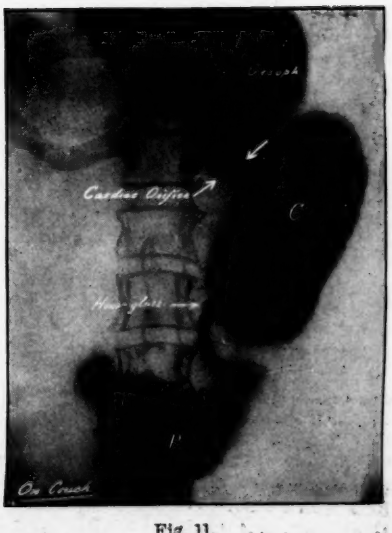

Fig. 11.

after gastro-enterostomy for pyloric stenosis. One consideration throws doubt on the explanation of cardiospasm as an occasional manifestation of intestinal stasis: one would expect to meet with slight degrees of spasm at the cardiac orifice in a fair proportion of well-marked cases of stasis. I am bound to admit this is not so; I observe the passage of the bismuth through the oesophagus in every gastro-intestinal investigation, and I find it very rare to encounter any difficulty whatever at the cardiac orifice in the ordinary run of stasis subjects.

My most recent case of cardiospasm, however, affords strong confirmation. A woman aged 39 had suffered from difficulty in swallowing, and regurgitation of food and mucus for two years. I found the oesophagus enormously dilated, presenting a large shadow in the right chest, as seen from the front without bismuth (Fig. 1). With bismuth, in the right anterior oblique view, the bismuth emulsion lay on the surface of the fluid already occupying the oesophagus, the long dividing line indicating its abnormally great diameter. The heavy bismuth fell slowly, and began to trickle through the conical cardiac orifice. The stomach was then shown to consist of two portions separated by a long, irregular hour-glass constriction (Figs. 10 and 11). The pylorus appeared normal in form, but only traces of bismuth were seen in the duodenum on this the only examination I was able to make.

The combination of cardiospasm with chronic gastric ulcer is new to me. I showed, quite recently,[1] how intestinal stasis causes gastric ulcer. For the notes of the operation performed a few days later by Sir Arbuthnot Lane I am indebted to his house-surgeon, Mr. F. Glendining:

There was an old, contracted gastric ulcer constricting the stomach, which was extraordinarily narrow and contracted—no doubt owing to the small amount of food that had passed into it lately. The duodenum was narrow, long, and pouched as if it had formerly been very dilated, but, again owing to small amount of food, had contracted. The appendix controlled the ileum (causing ileal stasis), and she had a very dropped large gut. The oesophageal condition was due to cardiospasm.

REFERENCE.

[1] Gastric Ulcer, *Proc. Roy. Soc. Med.*, 1913, vol. vi (Elect.-Therap. Sect., pp. 117–138).

DISCUSSION.

Dr. WILLIAM HILL (London) said that Dr. Jordan had shown some excellent *x*-ray prints of cases of general dilatation of the oesophagus, but he (the speaker) very much doubted that primary cardiospasm was often, if ever, the cause of the condition. Dilatation was the usual sequence of anatomic stricture, and also of kinking or angulation. It was possibly in some few cases the result of physiologic—that is, functional—stenosis of the phreno-cardiac portion of the oesophagus, as from paresis or atony, or from inco-ordination of the swallowing act, the lower end of the gullet failing to open out wide when food reached the level of the diaphragm. Evidence of primary spasm as the cause of the stenosis and of the subsequent dilatation was not in his (the speaker's) considerable oesophagoscopic experience forthcoming when an adequate investigation was made. The *x*-ray examination alone was not decisive, but must be supplemented by visual inspection through the endoscope, when a definite anatomic narrowing of the phreno-cardiac region would be found in nearly every case. In fact, the speaker had long been looking out for a genuine case of purely functional phreno-cardiac stenosis, and had as yet failed to find one, though he did not deny their possible existence. There were clinical difficulties in the way of accepting primary spasm as the predominant feature even in alleged functional stenosis, for the special mark of spasm in the alimentary tract, whether primary or secondary, was pain of the nature of colic or cramp, or else of stitch. It was significant that a painful cramp only occurred secondarily in these cases, if at all, when a portion of solid food got impacted in the stenosed region; it was not induced by "held up" liquid food. As a primary spontaneous colic was not a feature of alleged primary cardiospasm, it had been argued that the spasm which was postulated as the cause of functional phreno-cardiac stenosis was not a marked one, and that the condition was little more than a slight hypertonus or even a mere want of relaxation of the circular fibres. There was no demonstrable sphincter, and the assumed non-relaxation was rather of the nature of want of co-ordination in the act of swallowing, the phreno-cardia not opening widely enough to allow the food to pass. The latter act was probably a powerfully muscular one, and not a mere inhibition of the tone of the circular fibres, and, if this were true, failure to open up was much more likely to be due to functional paresis or atony than to a mere spasm without the characteristic pain. Furthermore, the fact that these cases of alleged functional spasmodic stenosis were usually relieved, either temporarily or for prolonged periods, by mechanical dilatation was strong evidence in favour of either inflammatory soft tumefactive stenosis or of fibrous stricture. Mechanical irritation merely increased spasm, and would only relieve those functional cases due to paresis by stimulation. He believed nearly all the cases of dilated gullet loosely attributed to primary cardiospasm would be found to be due to an anatomic lesion if properly investigated. *X* rays were invaluable for showing angulation and for outlining the dilated gullet and showing the area of arrest of the bismuth-impregnated meal, but radiography must be combined with oesophagoscopy and with a study of the clinical features of the case in order to arrive at an accurate diagnosis. The speaker also went on to emphasize the point that the employment of blind methods of diagnosis and treatment had been abandoned by modern experts, and it was hardly justifiable for practitioners with few opportunities of dealing with these cases to insert blindly bougies, etc., into a strictured gullet. With the oesophagoscope inflammatory tumefaction, associated with inflammation and venous engorgement of the stomach, peptic ulcers, and fibrous strictures, could be definitely seen, and treatment should be carried out through the endoscope, whether by bougieing or otherwise.

Dr. R. MURRAY LESLIE (London) wished to mention an interesting case of cardiospasm. A young man, 28 years of age, came home from South Africa with dyspeptic troubles. He explained that the food seemed to stick after being swallowed, and that in an hour or two, or sometimes immediately, it came up again. Dr. Jordan made an *x*-ray examination, and found a well-marked stricture at the cardiac end of the oesophagus and an enormous distension of the oesophagus above. It was found possible for as much as two pints of food material to be held in his oesophagus. The first treatment was thoroughly to wash out the oesophagus, but this made no difference to the condition. An oesophagoscopic examination was made, and a tiny quill was successfully passed after the patient had swallowed some silk thread. Before finally counselling operation special kinds of tubes were inserted, but ultimately operation proved to be the only course. On operation they were able to open up and pass a tube from below, and an ordinary large stomach tube was kept down. There was evidently no cicatrix, and the case was purely spasmodic. The patient succumbed to leakage from the gastric wound, and at the *post-mortem* examination there was found to be enormous distension of the oesophagus, which was almost the size of the stomach. The speaker was just able to get his little finger right through. It was inevitable to conclude that they had there a condition of primary cardiospasm, and that by giving a generalized anaesthetic it would have been quite possible to pass a tube. They much regretted that the patient had not been put deeply under an anaesthetic and this procedure carried out; but one learned from one's mistakes.

Dr. G. E. BOWKER (Bath) gave the details of an interesting case of cardiospasm. The first photograph, taken with bismuth in the oesophagus, showed the oesophagus with the appearance of a champagne bottle. The patient, a nurse, went under treatment in the various institutions in which she happened to be employed, and while at Bath she was able to go about her work by following out the usual procedure of washing out the oesophagus regularly. Ultimately she passed into the hands of Mr. Rushton Parker, at Liverpool, and there she went under operation, the stomach being opened through the abdominal wall, and the cardiac orifice of the stomach dilated through the opened stomach. She recovered from that operation, and afterwards did remarkably well, putting on weight, and

requiring no assistance. The last he saw of her was when a second radiograph was taken and showed an entirely different form of oesophagus, the shape now being something like that of a collapsed football. She had lately been operated on for appendicitis, from which she made an uneventful recovery, and she was still, he believed, sound and well and free from any cardiospasm.

Dr. A. F. Hertz (London) said that the Section was indebted to Dr. Jordan, because it was most important that this class of case should be recognized. Hitherto it had frequently been misunderstood. The condition was known to some extent years ago, and nearly every museum in London contained an example of an enormous so-called "idiopathic dilated oesophagus." On examining such examples it would be noticed that, although the oesophagus was enormously dilated and hypertrophied, the cardiac sphincter was not hypertrophied. If one had continuous spasm anywhere else one got hypertrophy. A spasm of the pylorus produced a hypertrophied pylorus, a spasm of the anus a hypertrophied sphincter ani, and so on. But in spite of the enormous efforts of the oesophagus to overcome the difficulty at the cardiac orifice, there was yet no hypertrophy of the sphincter. It was absolutely normal. The only possible explanation was that some question of inco-ordination was involved. One knew from physiological experiments made years ago that, in the normal process of swallowing, the moment that the contraction, passing down the oesophagus, reached the cardiac sphincter, this was relaxed—in just the same way as, with the contraction of the rectum in defaecation, the sphincter ani relaxed. But in certain cases the sphincter did not relax, sometimes owing to organic lesions, or tumefaction of the oesophageal mucous membrane, and at other times, he believed, to purely nervous causes. He recalled a case of a man of about 60, who had had absolutely perfect health, and who received a shock one morning at breakfast owing to the contents of a letter, and found himself unable to swallow. The speaker saw him shortly afterwards, and found that all the food was held up at the cardiac sphincter. A bougie was passed and the condition cleared up. This might have been a case of spasm, but the pathological evidence was all in favour of an upset of co-ordination. With regard to treatment, all must admit the possible dangers of the bougie. In the early stages of some cases with this dilated oesophagus the passage of a mercury bougie was, in his own limited experience, most successful. On the other hand, the cases in which he had not been successful were the more severe ones in which there was pouching. In those cases the bougie, on being passed, was apt to go against the pouch, and there was danger of perforation. One imagined that the only way to introduce the bougie in such cases was through the oesophagoscope. In the milder cases treatment with a mercury tube, which was a really safe and simple means, should be tried before the more elaborate one of swallowing the thread and passing the bougies over it.

Dr. J. Delpratt Harris (Exeter) said that spasm existed surely enough at the lower end of the oesophagus. Not long ago, while using the barium meal, he observed the barium stop a little way down in the thorax, and, watching further, he saw a process of extrusion take place, the barium coming out like clay. Whereas previously it had been going down slowly, it seemed afterwards to fall by the action of gravity. Without wishing to drag one subject into another, he would like to call attention to the two different sets of phenomena that occurred with the use respectively of barium and of bismuth. The great value of the barium meal was that, although scarcely as opaque as bismuth, it revealed the phenomena within a shorter period of time. This was an advantage, as it was impossible in private practice to keep the patient under control for twelve or twenty-four hours. The patient passed out of their observation within a few hours at the most, and anything which expedited the phenomena was of value.

Dr. Jordan, in reply, said that evidently cardiospasm was a most surprising and mysterious condition. The name had never appealed to him, but it was the name in prevalent use, and he thought he could not do better than avail himself of it. The dangers of the treatment were greatly diminished by the use of the silk thread referred to in his paper. There was very little fear of doing injury when adequate precautions were taken, and any careful operator ought to be able to prevent all danger of perforation. When the passage of the bougie was undertaken while the patient had a generalized anaesthetic, it was even more necessary to have the silk thread as a guide.

COLLARGOL INJECTIONS IN THE DIAGNOSIS OF DISEASES OF THE URINARY TRACT.

By W. Ironside Bruce, M.D.,
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Just as bismuth food in the diagnosis of the alimentary tract opened up an entirely new line of investigation and has yielded most encouraging results, so the use of collargol in diseases of the urinary system is now proving to be of the utmost value. Its application is, however, by no means so simple as bismuth food, for the solution has to be introduced through the urethra and ureter. For this purpose the cystoscope is essential, for by the aid of this instrument the ureteric catheter is introduced into the renal pelvis, and so the injection of collargol is made.

This may seem a simple operation, but success can only be obtained with much practice and a great deal of manipulative skill. The process of catheterization of the uterers and the collargol injection is the really essential part of the method, and in the hands of Mr. Thomson Walker, who has conducted all the examinations I have seen, was most successfully carried out.

Mr. Walker proceeds as follows: The cystoscope is introduced, the bladder distended with water, and the ureteric catheter passed into the renal pelvis. Having accomplished this, the bladder is emptied again, the cystoscope withdrawn, leaving the ureteric catheter *in situ*—a matter of some difficulty, for, if not carefully done, the catheter will be drawn out of the ureter. The patient is now quite comfortable, and the collargol solution, which is sterile and carefully warmed, is allowed to flow into the pelvis of the kidney from a glass serum syringe by the force of gravity only, a head of not more than 1 ft. being used.

Any suggestion of discomfort or pain complained of in the region of the kidney is an indication to stop the injection, and when this has been decided upon the end of the catheter is plugged, surrounded by cotton-wool, and strapped or bandaged to the patient's thigh. The plugging of the end of the catheter and the use of the cotton-wool is a practical point, for it must be remembered that collargol is dangerous material, for it is opaque to the *x* ray, and if it gets on to the couch, or any other part of the *x*-ray equipment, it is most difficult to get rid of.

The patient is now ready for the *x*-ray examination, which should be carried out with as little delay as possible. I employ the method which I use in ordinary renal investigations—that is, the patient is laid face downwards on the couch, with the tube arranged underneath, a compressor pad being placed under the abdomen.

At first sight it would appear that the compressor pad would cause considerable inconvenience, but in practice this is not so.

The method has been employed so far mainly for the early diagnosis of hydronephrosis, but in the future it may be found to give useful information in other conditions, for example, tuberculous or malignant disease of the kidney.

DISCUSSION.

Mr. Gilbert Scott (London Hospital) said that he had had quite a number of cases in which this method had been employed, and a very interesting condition had been discovered. One patient was shown to have double ureters and double pelves on both sides, and also a tumour in one kidney. There was a small point in technique which had given him some trouble—namely, the running down of the collargol on one side of the catheter. In one or two of the radiographs the collargol showed itself in the form of little bulbous shadows at intervals down the

line of the ureter. It was best to get everything in readiness before the collargol was injected, and to make the radiograph quickly.

Dr. Ironside Bruce said that so far as the difficulty of keeping the pressure up in the catheter was concerned, the procedure which he had employed in renal examination was to turn the patient on his face quickly after injection, and the radiograph could be obtained in thirty seconds.

X-RAY TREATMENT OF UTERINE FIBROID.

By Agnes F. Savill, M.A., M.D.Glas., M.R.C.P.I.,
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During the past four years I have had the opportunity of treating three cases of uterine fibroid by *x* rays. The notes of the first case were published in the *Lancet* of October 15th, 1910.

The cases showed a marked contrast in the general symptoms caused by the destruction of ovarian activity which was brought about by *x*-ray treatment. The more rapidly the treatment was carried out, the fewer were the constitutional symptoms.

My first case was a single woman, aged 43, who had suffered for many years from various nervous symptoms and vasomotor disturbances. Her pulse was always about 100, and she had frequent attacks of giddiness, flushing, and great prostration following any gastric upset. In 1900 she had profuse haemorrhage at the monthly periods. On examination she was found to be suffering from a fibroid uterine tumour. By 1909 the tumour had grown above the level of the umbilicus; her waist measured 36 in. Haemorrhage had continued severe and frequent, so that the patient was extremely anaemic and had to live a sedentary life. She refused operative treatment.

In May of that year (1909) I began treatment by the *x* rays—at first by Schönberg's method. By this method she had altogether some fifteen exposures, and on an average one-third of a Sabouraud dose at each sitting, measured above a filter of three layers of leather and two of tin-foil. In July and August the periods were unusually profuse and painful. In October of 1909 Bordier's method of treatment was commenced. At that time his method was to place a pastille beneath the filter directly on the skin, so that one measured the exact dose received by the skin. Each dose measured Tint O. Bordier, which is almost the equivalent of a Sabouraud dose. Three exposures a week were given for three weeks, and after a week or two had elapsed this series of nine was repeated. At each exposure I employed a filter of aluminium half a millimetre thick. In the evening, after an exposure, the patient complained of headache, mental depression, rapid pulse, and pain in the neck and ovarian regions. Consequently, after the first three or four exposures, she only permitted me to administer about half the correct dose at each sitting. During the first three months of treatment the patient had attacks of exhaustion and mental depression, with abdominal swelling, rapid pulse and gnawing pain. On these occasions I could feel that the tumour was softened and enlarged. When the symptoms subsided the tumour became hard and contracted. After a few months' treatment she had a slight oozing, which lasted for a fortnight, followed in a short time by a profuse period, and then amenorrhoea lasting for three months. Treatment was arrested in December, and resumed in April, 1910. I then gave exposures at intervals of four or five days, and a long rest in August and September. Treatment continued until October, 1910. She had, in all, some forty-eight sittings extending over eighteen months. Bordier says it is not safe to give more than forty sittings, but as this patient never had more than three-quarters of his dose, and usually had less, she was well under the danger limit. In no single sitting was a full dose given. During treatment the skin acquired a slight pigmentation which gradually disappeared. She has now, in 1913, telangiectases on both ovarian regions. Her health, however, is better than it has been for years, except for marked constipation which is an entirely new symptom with her. In this case the stimulated ovarian activity, brought about by the first doses of the *x*-rays, showed itself by profuse haemorrhage and prolonged oozing, signs which continued, more or less, for nine months after the commencement of treatment. It is to be noted, however, that even now (July, 1913), there is but little diminution in the size of the tumour.

My second case of uterine fibroid was treated by Bordier's latest method, as given in his paper which was read before the Royal Society of Medicine last summer (1912). The patient was under the care of Dr. Alice Corthorn, to whom I am indebted for information as to the history.

Miss J., aged 46, had suffered from menorrhagia for over six years. The period set in every three and a half weeks, and lasted about eight days, with very profuse flow and clots of blood. She complained of great weakness and a feeling of weight in front. Some bladder trouble was present, due to pressure by the tumour. When examined in February, 1912, the fibroid was the size of a small orange on the right of the uterus, pushing the fundus over to the left. Treatment by styptol, ergot, and hydrastis at first caused diminution in the amount of loss, but by May, 1912, the tumour had increased in size and the periods had become more frequent. In September the tumour was as large as a big cricket ball, lying in Douglas's pouch. On September 21st *x*-ray treatment was begun, and continued according to Bordier's rules until December 6th. Only twenty exposures in all were administered—one series of nine, one series of eight, and a cycle of three. Bordier gives nine sittings on nine consecutive days, but I was afraid to push the treatment quite so quickly. In the first two series of nine the sittings were given with one day's interval between each cycle of three. After the first series of nine the menstrual period was greatly increased. On the sixth exposure during the second series slight pinkness was noticed on each ovarian region. Treatment was suspended, and the pinkness rapidly developed into a dusky redness with slight swelling of the skin, which did not entirely disappear until several weeks had elapsed. The period following this second series was postponed almost a week, but when it did set in it was excessive in quantity. After an interval of over a month three further exposures were administered—one on the middle line and one on each ovarian line. Slight redness appeared on each ovarian region the day after the exposure, and treatment was arrested. There was then complete amenorrhoea for three months, when, during thyroid administration, another slight loss of blood occurred. Dr. Corthorn reported that examination proved that the tumour had disappeared by May, 1913. Pressure symptoms had disappeared during the first month of treatment.

The points of interest in this case are:

1. No symptoms of the menopause were present. The patient had no flushings and no exhaustion. Throughout the treatment she felt stronger than she had been for some time. This clinical picture is in marked contrast to that of Cases I and III.

2. The occurrence of a mild degree of radio-dermatitis.

A few words must be said with regard to this dermatitis. In 1909 Bordier recommended the method employed in my first case as absolutely safe. Nine exposures were given in three weeks. A sheet of aluminium (of from ½ to 1½ mm. in thickness) was employed as a filter, and the measuring pastille was placed directly on the skin *beneath the filter*. In his article of 1909, Bordier stated: "With the technique above described the patient runs no risk of dermatitis, the cutaneous effects consisting at most of an erythema with desquamation. The measurement of the dose at each sitting can be easily made, and security is obtained in the dosage of radio-therapeutic treatment." In 1912, however, he declared that this method was not always safe. He had found long-delayed trophic troubles and dermatitis.

To avoid all danger Bordier now recommends that nine exposures should be given on nine successive days, just after a menstrual period, each series of nine to consist of three cycles—one exposure over each ovarian region and one over the middle line. The measuring pastille is placed *on the top of the filter*. A filter of aluminium is placed over the ovaries, and is gradually increased in thickness with each cycle. For the middle line a thick 3½ mm. filter is always employed lest a median incision should be required later on. During treatment of the lateral region the median line is protected by a wide strip of lead. In small fibroids Bordier obtains a cure after two series of exposures. He employs a water-cooled anode tube of 9–10 Benoist penetration. The tube must be held very near the patient. He recommends that a breadth of four fingers only should be present between the glass of the tube and the patient.

As he did not say whether the filter should be laid in immediate contact with the skin, I placed the filter directly on the skin and earthed it by a wire leading to the hot-water pipes. It has been suggested to me by other *x*-ray workers that the dermatitis was due to secondary radiation from the metal plate. As Bordier allows so little distance between the patient and the tube (four finger-breadths), there could not be much space for felt or other material, otherwise the pastille placed on the top of the filter would be too far from the skin to be a true guide as to the dose administered.

During the first fourteen sittings I did not employ a water-cooled heavy anode tube. I employed one of Mr. Dean's small light anode tubes. It was very hard, with a spark gap varying from 14 to 18 cm., and the Benoist penetration when tested was always about 9–10. A current of 1 milliampère was run through. During the time of application, which varied from ten to sixteen minutes, the

milliampèrage remained entirely steady; only on one occasion when treating the left flank it rose to 1½ milliampères. In the last cycle I employed a water-cooled anode tube with all other details exactly as Bordier recommends. In searching for causes of the dermatitis, I discovered that the sheets of aluminium as used in England are not exactly the same thickness as those employed by Bordier. The plate which is sold as being 0.5 mm. aluminium is, in reality, in England, only 0.4 mm. Hence, I had employed 0.4 mm. for three exposures when I should have had 0.5. Again, in the copy of Bordier's instructions with which I worked, I discovered a misprint. In the second exposure of the flanks during the second series the correct thickness of the filter should have been 1.3 mm. In my copy this read as 1 mm. But allowing for these small errors, my filters were deficient over the ovarian regions by only 0.6 mm. I do not consider that this was sufficient to account for the resulting erythema and swelling. On several occasions during the treatment, knowing that 0.5 mm. aluminium was a very slight filter for the skin, I took the precaution of placing the pastille under, as well as above, the filter, directly on the skin as well as on the aluminium plate. When the tube was working in fifteen minutes I found that with a 0.4 mm. aluminium filter there was less than 1 Hampson unit of difference between the pastilles. I do not, therefore, think it safe to give a full dose with a 0.4 mm. aluminium filter at intervals of three days.

My third case was sent to me in May, 1913, by Dr. Alice Corthorn. She was a widow, aged 48, and had a history of tumour growth since 1907. In 1910 the periods became very excessive and irregular. In spring, 1913, the periods set in every fourteen days, and were very profuse. I decided not to risk a dermatitis, and therefore during the first two series (18 sittings) I gave only about ¾ Bordier doses, and employed thicker screens than advised by Bordier. I also separated the metal filter from the skin by a layer of felt. A pastille was placed *on the skin* and *on the top of the filter*, and both measurements noted. The skin became pigmented, but there was no dermatitis. The edge of the metal filter (which was earthed) rested on the thigh without any protecting layer of felt; and there she developed a line of dermatitis, even though that area was not the part on which the greatest amount of rays could fall. After the 18 exposures she had had under two-thirds of the dosage Bordier recommends; the ovarian function was stimulated, and a profuse flow ensued. Six further exposures were given in July. A filter of sodium tungstate lint was employed, and Bordier's full dose (Tint III by electric light) was administered on the top of the filter. On the skin the pastille twice registered 8 Hampson units. Dermatitis set in on that area three weeks later but slowly healed. Amenorrhoea was complete. During treatment the patient felt very exhausted and was troubled with flushes, and these continued for some time after the amenorrhoea.

Successful treatment depends on dosage; therefore I have made experiments to ascertain the relative value of different filters. I placed two pastilles above and below two adjacent sheets of aluminium, 0.4 mm. and 0.3 mm. thick respectively, and exposed the two at the same time. I jotted down the milliampèrage, Benoist, spark-gap, and time of every experiment, and wrote these notes on different sheets of paper daily, so that no subjective bias should prejudice the experiment. The results were then tabulated as in the diagram shown. This shows that 0.4 mm. aluminium retards only about one-quarter of the rays, while 3 mm. does not retard so much in proportion, but from three-fifths to one-half only. Bordier obtained very different results from his chemical method of measurement. He employed tubes of 9 to 10 Benoist from a water-cooled tube, and found that 0.5 mm. aluminium only allowed 55 per cent., and 3 mm. allowed only 14 per cent. to pass through. With my pastille measurements I found that when his Tint III was obtained above, the pastille beneath 0.4 mm. aluminium registered over eight of Hampson's units, four of which are equal to the epilation dose. Now Bordier recommends his Tint III by electric light as the constant dose to be measured over the filter. I find this Tint III is nearly equivalent to 12 Hampson units by electric light; hence, if the pastille beneath a thin filter receives a three-quarter dose, it is obvious that the skin receives more than the epilation dose (4 Hampson units).

The penetration from hard and from soft tubes differed little except when the tube was very soft indeed—2 or 3 Benoist. I then tested the relative value of screens made of felt (the method of Dr. Ironside Bruce) and of lint soaked in a saturated solution of sodium tungstate and dried (the method of Dr. R. Morton). Twelve layers of felt and eight layers of the sodium tungstate lint were tested at the same exposure, and it was found that when the pastille at half-way distance registered Tint B, the pastille under the felt showed three Hampson units, and the pastille under the lint only one.

Further experiments showed that twelve layers of felt were equivalent to four layers of sodium tungstate lint, and, if the lint was very heavily impregnated, two layers would give the same degree of retardation.

TABLE I.—*Registration of Pastille Measurements in Hampson Units.*

H = Hampson.

Date.	Type of Tube.	Ma.	Benoist.	Spark Gap.	Time	Thickness of Aluminium Sheets.		
						0.4 mm.	3 mm.	1 mm.
					Min.			
May 15	Small hard light anode	½	—	—	10	H 4* H 2+	H 4* H 1	
May 15	Water cooled heavy anode	—	—	—	12	H 3+ H 3	H 4 H 2–	
May 17	,,	—	—	6	—	H 4 H 3		H 7 H 4
May 17	Small hard light anode	—	10	—	9	H 5 H 4		H 5 H 3
May 17	Large light anode	—	—	6	15	H 4 H 3+		H 4 H 3–
June 2	Water cooled heavy anode	1	7	—	15	H 4 H 3	H 4 H 1	
June 2	Small hard light anode	1	10	—	8	H 5 H 4	H 5 H 1+	
June 6	,,	1	10	—	12	H 9 H 7	H 9 H 3	
July 4	,,	1	10	—	8½	H 5 H 3+	H 5 H 2	
July 6	,,	1	10	13	8	H 5 H 3+	H 5 H 2	
July 6	Large light anode	1	3	8	16	H 4 H 2	H 4 H 1	
July 6	Water cooled heavy anode	1	6	9	8	H 4+ H 3	H 4+ H 1	

* The top line in each experiment refers to the pastille placed above the aluminium filter, the bottom line to that placed under the filter.

TABLE II.—*Showing Penetration through Screens of Sodium Tungstate Lint and Felt.*

Pastille measurements in Hampson units.

Date.	Type of Tube.	Ma.	Spark Gap.	Benoist.	Time	Screen of 12 layers Felt.	Screen of Lint.*	No. of layers of Lint.*
					Min.			
July 10	Water cooled heavy anode	2-1	—	4	7	H 3	H 1	8
July 10	Light anode	¾	8	7	10	H 4	H 3+	4
July 10	Light anode	¾–1½	—	7-4	10	H 3	H 2	4

* Sodium tungstate lint.

TABLE III.—*Comparing Penetration of Screens of 3mm. Aluminium and Sodium Tungstate Lint and Lint.*

Date.	Type of Tube.	Ma.	Spark Gap.	Benoist.	Time	3 mm. Aluminium.	S. T. Lint.*	No. of layers of S.T. Lint.*
					Min.			
July 12	Hard light anode	1	16	10	10	H 8 H 1+	H 8 H 1+	4
July 12	Hard light anode	1	16	10	—	H 6 H 1	H 6 H 1	4
July 14	Water cooled heavy anode	1	13	6	6	Simple Lint H 5 H 4	H 5 H 2	4
July 14	Hard light anode	¾	13	6 to 7	7	H 5 H 4	H 5 H 2	4

* Sodium tungstate lint.

Plain lint formed a very slight barrier to the passage of the rays. Thus, when authors discuss their dosage and their screening methods, we shall in the future be able to understand the relative properties of their filters.

SECONDARY X RAYS IN CONNEXION WITH IONIZATION.

By John Delpratt Harris, M.D., M.R.C.S.,
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The year before last, when the Annual Meeting of this Association was held in Birmingham, I mentioned a case in which *x* rays given in unusual doses proved successful in causing shrinkage and eventual disappearance of a tumour containing bony plates—osteo-sarcoma. This occurred after the tumour had proved immune to smaller doses.

It is this intermittent action that has been occupying my thought and consideration in connexion with secondary *x* rays. There appears to be an inhibitive action in the patient, restraining the beneficial qualities of *x* rays, present at one time, yet absent at another; and further, cases of epithelioma of the auricle of the ear heal under *x* rays, yet the glands involved seem quite unaffected by them. The object of this paper is to show that this curative action seems capable of restoration by ionization in some instances.

In 1909 Mrs. B. was seen by me in consultation with a view to electrical treatment of a large fungating ulcer at the junction of the hairy scalp with the forehead. Three sittings of *x* rays were given, as the case was one of papilloma degenerating into epithelioma. I then represented to the family that it would be dangerous to the health to get rid of so large a mass by absorption, supposing *x* rays proved successful, and on October 13th I removed it by the knife. It was a bun-shaped mass, highly offensive, not less than 3½ in. in diameter. Centrally the bone was discoloured with a brown tint, but not eroded. It healed in very slowly from the edges, and a central patch, more or less red in colour, refused to heal. *X* rays were therefore applied—four sittings, two a week. Zinc ions, twice a week, next were used, and again *x* rays, a week apart. On several occasions complete healing occurred, only to break down again if there were any exposure to cold. As the wound gradually became smaller and divided itself into three or four smaller ulcers, zinc ions were introduced until the wound, becoming glazed over by a very thin layer of epithelial cells, refused to take the characteristic grey tint caused by the ions of this metal. *X* rays were then used several times in succession, and the wound healed. As smaller and smaller ulcers again formed, the same routine was followed, but complete permanent healing did not take place for three years.

It was this case that brought the idea of systematically using the two remedies to my mind. It occurred to me that possibly the zinc ionization acted in two ways: first, its own specific action, whereby rodent ulcers and spreading lupus were transformed into healing ulcers; secondly, the ions, which were substantial enough to colour the granulations, gave off secondary *x* rays on being acted on by primary *x* rays, and *that* in the deeper portions of the granulations. On this assumption the ions have been allowed to remain for three or four days or more before the use of the *x*-ray tube.

Miss E. was sent to me by a practitioner who had tried all kinds of medical remedies and local applications of formalin with some success for tuberculous ulceration of the end of the nose, a very superficial form of lupus, in which the cheeks were not affected. In this case *x* rays and ions of zinc were systematically given—the ions of zinc whilst the three or four small wounds were open and *x* rays as soon as they were too small or covered with crusts. Under this treatment the improvement was most marked. She is now well, but I still keep her under observation, as crusts occasionally form.

Mr. H. was sent in for lupus on the bridge of the nose of the scaly type, affecting the most superficial layers only. This gave way almost immediately to *x* rays, as such cases usually do. But on the upper lip, close beside the ala nasi of the right side, was an ulcerating papilloma. There were everted edges, with a deep pit centrally. At first *x* rays appeared to cause healing and inversion of the edges, but later this effect passed away and CO_2 was tried. This did not produce the benefit I quite anticipated, and after a few weeks zinc ionization was commenced. At this time the base was inflamed and the ala of the nose nearly involved, and I debated in my mind whether I had not tried these measures long enough and whether I ought to urge him to have an operation, necessarily disfiguring, performed with little delay. The cases already reported to you had made an impression on my mind, and I determined to try the method.

Two treatments of zinc ions, given a fortnight apart, were followed by two of *x* rays, with the best possible effect. The ulcerated surface healed centrally, having a heaped-up ridge around. The ridge flattened gradually from the outer edge, and finally the ridge nearest the ala flattened. A whitish, but thin, scar remains. I regard this case as a very convincing one, for *x* rays were given a full and long trial, and though successful at first, the case threatened to degenerate in spite of them. The ions of zinc supplied a means of regenerating their useful effect, which was never lost after.

I do not propose to read you further detailed notes of cases, but epitomize one or two others to show the class of cases this routine seems to suit best.

Sarah H., a woman of 72, had an unusual form of ulcer of her left nipple. It was on the surface, and did not produce any loss of substance. Ionization with zinc always healed the ulcerated portion, but fresh ulcers formed, and so the net result was but slight, until *x* rays were given more or less alternately, when healing still took place, but it ceased to break out afterwards.

Tuberculous ulcers healed readily under this combined treatment in a girl with tubercle of the skin near the elbow-joint.

Captain Henry J. was a case of small but penetrating rodent ulcer of the right ala of the nose. He was over 80, and came from the same town the case with papilloma came from. He was put on the combined treatment with quick and complete healing. Less than a year was sufficient to heal this ulcer, and there is no sign of its breaking down, although he occasionally comes to report himself.

I have had one or two failures where cases have not responded. These have been in cases in which the condition has been more or less eczematous, and in which zinc ions have difficulty in penetrating deeply. But where granulations exist and deeper penetration can take place, the effect appears most striking.

Even the deeper tissues seem to respond to this treatment, for in a hospital case a man came with caries of the cartilages on two ribs with sinuses running underneath the thin edge of the greater pectoral muscle. There was more than one superficial ulcer also, each about the size of a threepenny piece. He was at first treated with *x* rays, which appeared to heal the superficial ulcers. Finding the sinuses disinclined to heal, the man was taken into the hospital and the sinuses thoroughly explored and scraped. He was next ionized with zinc, which quickly healed the superficial parts; and then *x*-rayed, and in a very few weeks this alternating treatment produced complete healing of the sinuses and superficial ulcers.

My belief, therefore, is most strong that *x* rays alternating with zinc ions will produce a better result than either of them singly, and I suggest this is due to secondary rays, not from the tissues, but from the ionized metal already introduced into the deeper tissues. I claim also that the ions of zinc have the property of supplying a means of resuscitating the effect *x* rays have in healing an ulcer, when that has been exhausted.

Should this be found a common experience, it will, I think, open a door for future experiment. On this occasion I can only indicate a line of research.

A most severe case of actinomycosis in a young girl came to the electrical department, and it was proposed to see whether *x* rays would be of use. When first she came down it appeared as though she scarcely had strength to go through the exposure. The left side of her face was greatly involved, together with the neck. Her temperature was high—namely, 104°—and it was only with the aid of a whole-length wheeled couch and lifts that she could be brought down at all. Potassium iodide had been pressed until her stomach could digest no larger dose. Her kidneys were deluged with potassium iodide and, we may assume, so was her blood. The first few exhibitions of *x* rays produced a slight but favourable effect. But her feverish condition and quick pulse still seemed to demand more stringent and hasty measures. I felt I could scarcely do any harm in a case so desperate, and injected a solution of potassium iodide into the diseased tissues in many places at one sitting, thinking to get the salt into more close proximity with the spores, and especially that excretion by the kidneys might be delayed. I then immediately *x*-rayed the parts. After many such sittings this was followed by recovery, slow but complete. Combined treatment was therefore given in this case also, but with iodine instead of a metallic salt.

One case of this kind cannot prove much, but taken in connexion with those in which the ions of zinc have been used, it may, I think, call for an examination of the effects of other bases introduced into the tissues, capable of giving off secondary rays.

Quinine, we know, is visibly fluorescent. Salts of strontium and barium fluoresce under certain electrical stimuli from the + electrode, and but slowly part with that condition.

The behaviour of iodine, bromine, salicine, magnesium, and other elements has not been investigated in this connexion, so far as I know.

The point which it is required to know seems to be as to the nature of the laws that govern secondary *x* rays. Do secondary *x* rays occur from substances introduced into our tissues by ionization? Have they any differential therapeutic effects? Secondary rays certainly do occur from substances introduced by ordinary means, such as injection. Probably we all have seen patients brought to us for bismuth meals, followed by *x*-ray examination, who have been loud in expressing how benefited they have felt by them. We have elevated our eyebrows and remarked on the influence of the mind on the body. But the case may be otherwise, and secondary *x* rays from bismuth during the examination may have been an important factor.

DISCUSSION.

Dr. G. E. BOWKER (Bath), after congratulating Dr. Harris on his results, and also Dr. Hernaman-Johnson on his scientific exposition of this combined method on the occasion of the annual meeting at Birmingham, said that he himself had had very marked and decisive successes with the use of the zinc ion precedent to *x*-ray irradiation. He mentioned a case of extensive rodent ulcer which was treated on each occasion with zinc ionization twenty-four hours before *x* rays were applied, and the result was a completely sound epithelial surface, with no recurrence up to date of the malignant growth.

Dr. W. HARWOOD NUTT (Sheffield) had used the combined treatment systematically for rodent ulcer, epithelioma, and various forms of lupus. Rodent and other forms of ulcer had given way to the combined treatment after they had refused to yield to any agent taken alone. One case of rodent ulcer in the inner canthus of the eye in a man of 67 had been ameliorated by *x* rays once, but the rays never appeared to be of any benefit subsequently until they were combined with zinc ionization, when the case healed up quite well.

Dr. DIRK DE VOS HUGO (Worcester, S.A.) confirmed Dr. Harris's experience of the combined method. He never treated cases of foul ulcer except by this procedure. He instanced its value in a case of very bad epithelioma of the lip which had resisted *x*-ray treatment, and also one of rodent ulcer of the ear, in which the cartilage was affected. In the latter case two surgeons recommended amputation of the ear, but by the use of the combined method for only six weeks the ulcer completely healed up.

Dr. F. FOWLER (Bournemouth) did not agree that it was advisable to use ionization as a routine method before *x*-ray treatment, even in cases of chronic ulceration. The treatment was generally painful, and should be kept for cases in which *x* rays seemed to fail. He believed that failure was often reported because unfiltered rays had been used, and in such cases, of course, the rays should be filtered.

Mr. J. HALL-EDWARDS (Birmingham) said that his experience with the combined methods had lasted for a considerable time, but he got quite as good results by using secondary radiation from silver. His practice was to cover over the wound with freshly precipitated silver, and the procedure was painless, and scarcely added at all to the elaboration of the treatment.

Dr. F. HERNAMAN-JOHNSON (Darlington), who was asked to speak in the absence of the reader of another paper on an allied subject, referred to the paper read to the Section by Professor Barkla, in which he laid a fair amount of stress upon the advantage of being able to get soft and readily absorbed radiations. These radiations were useful, providing they could be generated in the right place, but it was of no use trying to use pure specific secondary radiations from a zinc or copper plate, because these soft rays, unless they could be brought into direct contact with the tissues, did not appear to have any action at all. One could certainly cure skin lesions by going on for a sufficient length of time with a plate used as a "reflector," but the cure would not be due to the secondary rays generated by zinc or copper, it would be due to scattered primary radiation of the same quality as the primary exciting beam. Therefore, if it was desired to use zinc and copper radiation, which was only capable of penetrating a fraction of a millimetre of tissue, the only effectual way was to introduce the ions into the interior of the cells. That was really the only satisfactory way in which materials of low atomic weight could be used for the purpose, but if metals of higher atomic weight, giving a radiation capable of penetrating a centimetre or more, were substituted, then it was possible to make use of the method of powdering the surface as Mr. Hall-Edwards had described. It was necessary to distinguish very carefully between the effects of the primary and secondary rays, and in view of the fact that the whole of the secondary radiation from, say, bismuth could be stopped by a sheet of paper, as he had demonstrated by a photographic experiment, it seemed rather far-fetched to attribute any action on cancerous glands to the effect of the secondary rays from an insoluble bismuth salt scattered in their neighbourhood. In spite of what the physicists might say, it was true that the harder the rays one used and the more heavily filtered they were, the greater was the physiological effect. These hard rays were valuable, not only for deep, but also for surface structures, and it should be remembered that even if the organs scattered the rays, a large portion of them must be scattered back again by the surrounding organs, and thus finally absorbed. He hoped they would not be induced to waste time by using pure secondary rays from plates set at angles, because, if these were used with the object of obviating a deep effect, there would be no effect at all, save such as was produced by the scattered primary rays, and if one went up the scale to silver and tin there was not much advantage over and above what was obtained by using a tube of approximately the same penetration. The speaker added, apropos of Dr. Harris's paper, that, if one's simpler measures failed, one was quite justified in resorting to ionization. He had had many good results in such cases, which had been obstinate or unyielding to *x* rays in the ordinary way. On the whole, the progress of secondary radio-therapy in three years was rather disappointing, but if one definite method were added to their armamentarium, it would be quite sufficient to reward them for their trouble.

Dr. DELPRATT HARRIS, in reply, acknowledged his indebtedness to Dr. Hernaman-Johnson and also to Dr. N. S. Finzi for certain of the ideas underlying the work recorded in his paper.

THE RADIO-THERAPEUTIC TREATMENT OF GYNAECOLOGICAL CASES AT FREIBURG.*

By Dr. C. J. GAUSS,

Director of the Gynaecological Clinic at the University of Freiburg-i.-Br., Germany.

I FEEL specially honoured in having been afforded, through the invitation of the President of the Electro-therapeutical Section, this opportunity of addressing you on a subject which is undoubtedly one of the most interesting in the whole of modern medical science. In view of the shortness of the time at my disposal I must naturally confine myself within narrow limits. You will, therefore, pardon me if, after a very brief reference to the development of radiation therapy, I proceed directly to the main portion of my subject, in which I shall deal with the technical methods employed and the results achieved in the Freiburg University Gynaecological Clinic.

It is, of course, impossible for me in what follows to refer to every kind of ray treatment. When, therefore, I speak of radio-therapy, I shall understand only treatment with Roentgen rays, radium, and mesothorium. Nor could I expect to deal fully with all the diseases which we have treated by Roentgen-radium methods in our clinic. I shall leave out of account tuberculosis, various inflammatory conditions, pruritus, dysmenorrhoea, goitre, and other affections, and shall speak only of

* This paper was read at a joint meeting of the Gynaecological and Electro-therapeutical Sections.

Roentgen and radium methods as applied to fibroma, metritis, and cancer.

Radio-therapy was for a long time purely a surface treatment, the reason for this lying in the nature of Roentgen radiation. The Roentgen rays, like those of radium, are a compound of rays of different penetrations, in which, however, the soft element predominates. These soft rays have but little power of penetration, and their healing function, consequently, is confined chiefly to the body surface.

With the advent of knowledge that the hard rays could penetrate much more deeply, came attempts to employ these rays to a greater extent. This end was attained by the use of high vacuum tubes, and by placing metal filters of moderate thickness between the radiation and the tissues. The results were improved by this procedure, but they did not come up to expectations. As a proof of this, it may be sufficient to mention the fact that the French, always to the fore in this work, speak of a surgical radio-therapy, and, in agreement with the views and experiences of workers in other countries, hand over complicated cases, presenting large tumours and excessive anaemia, to the surgeon's knife.

At this point our own researches began. Relying upon the fact that by the use of thick metal filters a harder and more penetrating radiation could be secured than was possible by merely raising the vacuum of tubes, we treated, for purposes of comparison, young bean plants and tadpoles, both with strongly filtered and with quite unfiltered rays. The results were evident. The growth of the plant was arrested much more quickly by the filtered than by the unfiltered rays, and the tadpoles exposed to the filtered rays were killed during the xposure, whereas those irradiated with the unfiltered rays did not die until several hours later.

The practical application was obvious. It must be possible, we concluded, to obtain better penetrative action with filtered than with unfiltered rays, provided that a sufficiently large volume of the hard rays could be introduced into the body. We therefore projected the Roentgen rays through an aluminium filter 3 mm. in thickness, and strove to increase to the greatest possible extent the doses of these hard rays. Here we were helped principally by an application of the French "cross-fire" method, which we employ in the following way: As large a number of special fields as possible are marked off on the body of the patient, and through each of these fields—from thirty to fifty in all, on the abdomen and on the back—the *x* rays are introduced into the depths of the tissues, while the neighbouring fields remain covered. In carrying out this procedure we made a very useful observation which revealed the fact that the skin will stand a double or triple dose of filtered rays without danger of burning. We were able, therefore, without any difficulty to introduce into the body through the thirty or fifty above-mentioned fields from 600 to 1,000 × units (Kienböck measure*) by irradiating each field with 20 ×. The dose so applied represents a radiation series, which after periods of from two and a half to three weeks is repeated in exactly the same manner, until the appearance of amenorrhoea shows that the healing process has begun. Finally, as an extra precaution, one or two additional series are given in order to prevent any possibility of recurrence.

The rapid appearance of favourable results depends essentially upon the speedy application of the ray dose. It has been established experimentally that the effect of a given amount of rays varies with the intensity of its application. In the case of a large dose applied for a short time healing sets in quickly, whereas the same dose extended over a long period tends, on the contrary, to aggravate the symptoms of the disease. We are able to obtain such an intensive concentration of large doses within short periods of time by irradiating the various fields in quick succession, by reducing the distance of the *x*-ray tube from the body to between 15 and 20 cm., and by increasing the charge of the tubes to 5, 7, and even 10 milliampères on the secondary current circuit, such an increase being rendered possible by the use of instruments constructed expressly for our purpose. Hitherto our expectations have been justified by facts. By means of large doses, applied in as short a time as possible, we have succeeded in curing all the patients we have treated for fibroma and metritis. Amenorrhoea has set in, followed by complete disappearance or marked shrinkage, or at least arrested growth of the tumour. Latterly the average time needed to attain this result in the case of about three hundred patients with fibroma or metritis has been about eight weeks, the total dose in each case being about 1,800 ×.

It goes without saying that we did not venture to apply such large doses in non-malignant cases before we had made preliminary experimental investigations in cases of inoperable carcinoma. It will doubtless be known to you that in the course of these experiments we succeeded, by means of large doses of *x* rays, in influencing a number of hopeless cancer cases to such an extent that, although formerly carcinoma most certainly existed, it was no longer possible by a deep excision to detect the presence of cancer cells. One question was of special significance in the question of cancer cure by means of deep *x*-ray therapy. This was an extensive inoperable carcinoma of the stomach, whose existence was established by a laparotomy. As the result of *x*-ray treatment, it disappeared entirely for macroscopic vision, and microscopically was shown to be reduced to the minimum. We were, nevertheless, forced to admit that it was extraordinarily difficult, by the employment of *x* rays alone, to induce a permanent effect at any considerable depth. We therefore conceived the idea of applying radium simultaneously. The radium capsule does not merely project its rays forward, as does the *x*-ray tube, but distributes them in all directions. Moreover, radium rays, by reason of their proportion of extraordinarily hard gamma radiation, possess much more penetrating power than Roentgen rays, and it should be possible, we considered, by isolating the pure gamma rays of radium or mesothorium, to improve upon the unsuccessful attempts of other authors. The isolation was obtained by the Dominici method, using strong metal filters so as to cut off all but the gamma rays, and then introducing sufficiently large and hitherto untried doses of these rays to the seat of the disease. It was, of course, necessary first of all to find out the size of dose which the healthy organism could bear without injury.

As the result of laborious experiments we determined this for each of our capsules, and then proceeded, by the use of cross-fire, to treat a carcinoma with as many hard gamma rays as appeared in our experience permissible. In this way a radiation treatment has been rendered possible which, both in duration and intensity, was formerly undreamed of, and which, though varying with special circumstances, may involve a series of radiations extending over a period of from seventy to two hundred hours and the continuous use of from 400 to 800 mg. of mesothorium. The results have exceeded our expectations. Large ulcers have disappeared rapidly and been replaced by extensive webs of scar tissue. Bleeding, discharges, and pain have ceased at the same time, and the strength of the patients has improved markedly. Up to the present we have to record 28 such cures out of 82 cases of carcinoma treated purely by radio-therapy.

It is, of course, impossible to say to what extent these cures may be regarded as permanent. Our first case, treated eighteen months ago, has hitherto shown no signs of recurrence. If it should turn out that radio-therapy can offer only a palliative treatment for carcinoma, still it would be the best means which we know of as yet for brightening and lengthening the lives of the patients whom hitherto it has been impossible to help in any satisfactory way. But we have good reason to hope that our results will be permanent. In this connexion we may mention an interesting fact. Investigation has shown that of the patients operated on in our clinic for carcinoma of the uterus, almost all those not subjected to radio-therapy after operation died subsequently from recurrence, while of 21 patients who underwent regular radio-therapeutic treatment 20 have remained free from recurrence. In the one outstanding case all traces of the patient have been lost.

These facts furnish food for thought. They show that radio-therapeutic treatment must be repeated at certain intervals if the cure at first effected is to remain permanent.

I should mention, in conclusion, that we use mesothorium also in the treatment of non-malignant diseases

* 10 Kienböck × units are equal to 5 Hölzknecht, or Tint B, Sabouraud-Noiré.

of the female genital organs, especially in fibroma and metritis. The method of application is quite analogous to that employed in *x*-ray treatment. At intervals of three weeks a mesothorium capsule containing at least 50 mg. is introduced into cervix, uterus, or vagina, the strongly filtered rays being allowed to work as long as possible from this centre. By the use of this method two or three sittings have sufficed to cure all cases treated up to the present—that is to say, they have brought about shrinkage or disappearance of the tumour and cessation of menstruation, irrespective of the patient's age or of the size of the tumours or of the degree of anaemia.

In spite of such successes, however, we retain the Roentgen treatment in non-malignant gynaecological diseases, only combining it with the mesothorium treatment in specially complicated cases. Such a policy is, however, dictated only by economy, as we know that radium and mesothorium rays are much more valuable than Roentgen rays for the treatment of cancer.

DISCUSSION.

Dr. DEANE BUTCHER said he was anxious to know what was the area of the *porte d'entrée* through which Dr. Gauss projected the rays. A great discussion had arisen in Germany on this matter. It was so difficult to compare the dosage given by two different people, for the reason that the *porte d'entrée* was not definite. In the future there was some reason to suppose that doses would be estimated by the number of units X absorbed per square centimetre of the *porte d'entrée*. Then they were indebted to Dr. Gauss for a new idea, namely, the concentration of *x*-rays, not only in place, but in time. That was an absolutely new departure in *x*-ray treatment. They had been accustomed to spread their treatment over a long period, and by that measure the pathological cells seemed to get accustomed to the insult of the rays, if he might say so, and the reaction to be correspondingly less. He would ask a further question as to the method of cross-fire. The idea of cross-fire was very fascinating, but the thought occurred to them every now and then as to the possible inaccuracy of that cross-fire. And it seemed to the speaker that there must be another factor involved, that is to say, the attack on the fluids of the body, the lymph and the blood, in addition to the local action on the ovary itself. Then there was another point with regard to which he had hoped to learn something from him, namely, the hardening of the ray by filtration. The average hardness of the ray-beam could be increased by such a means, but did he understand Dr. Gauss to say that by any method of filtration they could increase the rapidity of the ethereal vibrations? The action of mesothorium was little known in this country, but he would like to hear from Dr. Gauss some appreciation of the particular advantages of mesothorium as compared with radium. The great merit of Professor Gauss's method seemed to be that he had increased the thickness of the aluminium shield. This consisted of a 3 mm. thickness of aluminium, and with such a filter one might go on for as long as necessary and the skin would not be injured. That in itself was a very great advantage. As to the question of burns he gathered from Dr. Gauss's book that under his new technique the burns were practically negligible. But how much did such a statement involve? What about the late reactions? He himself believed that the sudden attack by *x*-rays was less likely to burn than the practice of using smaller but unknown doses and extending them over an indefinite period of time.

Dr. HANS SCHLIMPERT (Freiburg) said that in the Freiburg clinic the number of operations for gynaecological conditions had diminished steadily. The results in every case by the method expounded by Dr. Gauss were good, even to the critical and jealous eye of the gynaecological surgeon. Then, again, there was no mortality, and, furthermore, there were no conditions, such as adhesions, etc., which caused trouble after operation. The only after-effect was a slight headache and sickness. In the Freiburg clinic the radiation method of treatment had been extended to carcinomatous conditions, and the results were very encouraging. The results of operation in malignant conditions, especially in sarcoma, were so unsatisfactory that the results with *x* rays were all the more welcome as manifesting a great improvement.

Dr. DELPRATT HARRIS (Exeter) asked whether the rapid absorption of large tumours was not dangerous.

Dr. ABBE (New York) asked whether the pedunculated fibroid yielded to this method of treatment.

A lady member asked what method Dr. Gauss employed to isolate the ovaries.

Mr. BECKWITH WHITEHOUSE (Birmingham) wished to know whether it was actually a fact that large interstitial uterine fibromyomata would disappear under the action of *x* rays. Furthermore, he asked whether any degenerations occurred, such as fibro-cystic change, with possibly later suppuration. If this was so, as it was in the days of the electrical treatment by Apostoli's method, then he preferred to regard operation by myomectomy or hysterectomy as the better procedure. On the other hand, if, as he had just heard from Professor Gauss and Dr. Hans Schlimpert, there was nothing but a succession of brilliant results and no disadvantages, he was afraid that gynaecological surgery had fallen on evil days.

Dr. GAUSS regretted that his knowledge of English was not sufficient for him to reply adequately on the discussion. With regard to the size of the *porte d'entrée*, he explained that they took a sector of a circle as a unit of area, the triangle measuring 6 sq. cm., and divided the abdomen into these sectors. With regard to the concentration of *x* rays in time, he found distinctly a better result when the same dose was compressed within a shorter time. The necessary dose could not be given in one series, because there must be two menstrual periods. With regard to the hard rays, the highest degree of hardness could not be reached without filtration, because of the intermixture of soft rays, which he thought it advisable to filter away. He was not able to compare radium with mesothorium precisely, but he had no doubt that the difference between the two agents was slight. He had had no burns which could be traced to any fault of technique during two and a half years, and during that time also he had seen no belated reactions, although Dr. Albers-Schönberg had said that the apprehension of such reactions could not be definitely laid aside until after six years. He had treated two or three cases of pedunculated fibroids, and had seen no bad results, nor had he observed any instance of danger to the patient from the absorption of the tumour masses.

THE TREATMENT OF RINGWORM BY X RAYS.

By GEORGE B. BATTEN, M.D.,
Honorary Radiologist, Home and Infirmary for Children and Women, Sydenham.

MY excuse for reading a paper on such a commonplace subject as ringworm is twofold: first, because I believe that I have had a longer (about eleven years) experience of its treatment by *x* rays than any one else in England, and secondly, because I wish to describe to you a modification of the ordinary modern technique, which I have devised, and which I hope will be found a help to other workers.

Ever since 1886, when I began practice in Dulwich, I have taken a great interest in ringworm, and I treated and cured many cases by the older, very tedious, and rather unsatisfactory methods. In 1900, hearing of the accidental production of baldness by *x*-ray treatment of lupus, I exposed a small portion of my left arm to *x* rays and succeeded in producing a temporarily bald patch on it without any ill effects. During the following two years I tried the effects of *x* rays on a few cases of ringworm of the scalp, but as I had to guess at the whole details of exposure and dose, the first cases were not very satisfactory, the hairs coming out better around the ringworm patches than in them. Dr. Norman Walker then suggested verbally to me to try ten-minute exposures. I found that six such exposures on alternate days produced the desired baldness. I referred to this treatment in a paper which

I read, on the uses of alternating current, to the British Electro-Therapeutic Society in March, 1903.

My first successful case was begun in November, 1902, and finished early in January, 1903; I show you a photograph of this case, taken some months afterwards, when the hair had grown again. This case was followed by many others, the results of which I published in the *General Practitioner* in 1904.

I wrote a fuller paper in November, 1904, which, with photographs, was published in the *Archives of the Röntgen Rays* in August, 1905. In this is shown what I believe to be the very first case in which the whole head was treated. This was done in the second half of 1903. In this case a portion of the scalp over the left ear remained somewhat bald for over a year, and up to 1907 the hair at this place was thinner than elsewhere on the head.

In 1905 Sabouraud and Noiré devised their pastille method of measuring a single effective epilation dose, and during the last few months of that year I began to use their method—at first exposing the head through holes cut in an ordinary school cap which had been covered with white lead, or else by exposing one part of the head, and, after it had become bald, covering it with a portion of such a cap, and exposing other portions in the same manner.

In December, 1906, I devised and tried an overlapping open method, that is, one which obviated the necessity of protecting devices on the child's head. I show you some photographs of the first case so treated, which was most successful. I managed this by moving the head and the *x*-ray tube about, and succeeded in epilating all except the areas above the ears by two such exposures. I treated several cases by this overlapping open method, but they were not all so successful. I published a paper on this method in the *Archives* in July of 1907.

Soon after that Dr. Agnes Savill tried the overlapping open method, and then Drs. Adamson and Critchley in this country and Dr. Kienböck in Vienna much improved the overlapping open method and invented the 5-in., 5-area naked overlapping method, now almost invariably used. Sabouraud's pastilles and distance were adhered to, the distance being measured by transparent wooden rods, which are usually called Kienböck's, but which I prefer to call Adamson's, because I believe that he and Dr. Critchley used them first.

This method is a very safe and satisfactory one, but it entails the pastille being at least half an inch from the glass of the tube, and therefore at least 3 in. from the anticathode, and the child's head must therefore be at least 6 in. from the anticathode. At this distance, with an average apparatus and an average tube, one area takes from 7,000 to 12,000 interruptions of from seven to fifteen minutes—that is, thirty-five to seventy-five minutes for the whole five areas. If many cases have to be treated, this is rather tedious. I have had tubes so well exhausted, so hard and so seasoned, that they will change the pastille in 5,200 interruptions, or under six minutes, but one cannot count on having a regular supply of such tubes.

To get over these difficulties, Dr. Hampson devised a method for shortening the time required by putting the pastille on the child's head and bringing the head to within about 4½ in. of the anticathode—that is, the "half-time distance." He invented a tube shield to suit this method, and also his radiometer, which divides Sabouraud's tint "B" into sixteen tints. When the pastille on the child's head has changed through four of these tints, the head has had an epilation dose. I have worked with his method, and find it efficient and quicker, but in my opinion it has two disadvantages: that it is very difficult for most workers, even in the proper artificial light, to match accurately all the requisite tints; and that without some other control it is hardly safe enough. In my opinion the safe margin for error above and below the correct dose is not more than 12 per cent., and the difference between, say, tint 4 and tint 5 is 20 per cent. of the correct dose. I may also mention that most of the shields made for Dr. Hampson's method have not a large enough hole, which should be 5 in. in diameter, whilst they are often not more than 4½ in.

To overcome these disadvantages I devised a modification in technique, which I think combines the safety of the Adamson or Kienböck-Sabouraud method with the rapidity of Hampson's. I have had a wooden tube shield made with a full 5 in. aperture, and with transparent distance rods of such a length that by simply removing them with their holder and putting the child's head at the level of the aperture one moves it from the "full-time" Sabouraud distance to the "half-time" Hampson distance; for instance, with a 5 in. (or 13 cm.) tube, the rods are 1¾ in. in length vertical measurement; with these distance rods of mine in place, the child's head is 6 in. from the "anticathode"; with the rods removed, the distance is 4¼ in., that is, the half-time distance. I have also devised a very simple pencil-like pastille-holder, and this is inserted radially to the anticathode.

With my rods in place I put the pastille at Sabouraud's half distance, that is, 3 in. from the anticathode. When the rods are removed and the head moved to the nearer distance the pastille distance is kept the same (3 in. from the anticathode) but a new pastille is, of course, used.

I use a small piece of bandage across the opening of the tube shield. This is kept in place by being fastened to pieces of bonnet elastic with safety hooks to the rim of the shield.

My procedure on any given day for the treatment of cases is as follows: I expose the area over the right ear and the area at the nape of the neck of the first child at the full distance and note the details of ampèrage, voltage, milliampèrage through the tube, and the equivalent spark-gap, and I find how long and how many interruptions the tube is taking to change a pastille to tint "B." Then I remove my distance rods and place another area of the child's head up against the bandage—that is, 4¼ in. from the anticathode—insert another pastille at the same distance, and set the tachymeter for half the number of interruptions, or half the time that was taken with the child's head at the full distance—that is to say, if the tube took 10,000 interruptions with the rods in, I set the tachymeter for 5,000 interruptions with the rods out. I do the three remaining areas of the first child at the half-time distance. With the second child I put my rods in again and do an area at the nape of the neck, again noting the number of interruptions required for tint "B." I remove the rods and do the four remaining areas at the half-time distance. I repeat this for all the other cases if the tube conditions remain fairly constant.

Of course, while dosing the areas at the half-time distance the pastille will only undergo a change equal to half of tint "B," and this I am gradually learning to recognize. However, I leave the same pastille or some portion of it in for two such half-time distances, and the tint then produced is the same as the Sabouraud tint "B," and serves as a useful and efficient check to one's dose calculated from the number of interruptions or the time occupied.

I had a wooden tube box made to suit this method, and Messrs. Newton and Wright lined it with protective rubber and made a slide with my distance rods on it. Since then they have made me one in papier-mâché, lined with opaque rubber, which I show you. You will see from this photograph that it is quite opaque to *x* rays. Messrs. Newton and Wright are now making these tube boxes in wood lined with opaque rubber, opaque papier-mâché, and also in glass, which is the best of all.

I pass round a few photographs of a child's head treated twenty-six days previously by this method, and I do not think that any one can tell which areas were treated at the full-time distance and which were done at the half-time distance.

I contend that my method is at least as safe as Sabouraud's and more than a third of the time quicker. If the tube is taking ten minutes for each area, it saves fifteen minutes on the first head and twenty minutes on each subsequent head treated—an important consideration for both the patient and the doctor. Only one—or, if one wishes, two—tints have to be learnt, and I believe that mine is a distinctly safer method than Dr. Hampson's.

With regard to general results: Before we had Sabouraud's pastilles in 1905 I had one case with complete baldness over an area the size of a florin and two cases of partial baldness, and also one case in which the hair on the front part of the head of a fair-haired boy did not return for over two years, when, however, it grew well. Since I have used the pastilles I have never had any cases of permanent baldness nor any other ill effects to the patients, and I have had exceptional opportunities for seeing and following up the cases. My only trouble has

been that during my ten and a half years' experience occasionally I have had to use slow tubes for short periods, and I have underdosed some cases and had to *x*-ray them over again after an interval. I have come across two cases this year, in which, even after three times of exposure to *x* rays, I have failed to cause proper epilation. I should like to ask members what is their routine treatment after dosage by *x* rays, and especially what they do to help to get out the very short ringworm stumps in cases where the dose has perhaps been not quite long enough.

SUGGESTED STANDARDS IN THE X-RAY EXAMINATION OF THE ALIMENTARY CANAL.

By ARTHUR F. HERTZ, M.D., F.R.C.P.Lond.,
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OPINIONS still differ as to the normal variations in the position of the stomach and colon, and the rate of the passage of food along the alimentary canal. In order that the *x*-ray reports of gastric and intestinal cases should be as valuable as possible, it is very desirable that there should be greater unanimity on these subjects. The differences of opinion depend in part upon differences in technique and in part upon insufficient experience in examining normal individuals. I propose to-day to make suggestions in order to lead to a discussion, which I hope will result in the laying down of a standard in technique and a definition as to what constitutes the normal.

1. *The Standard Meal.*—The appearance of the stomach varies greatly according to the amount of food which is present in it. In order to obtain comparable results the bulk of the meal should always be the same, and it is also desirable to have its composition fixed, as there is no doubt that variations in the proportion of its constituents result in variations in the activity of peristalsis and the rate of evacuation. In my opinion it is also necessary that about the same quantity of the same opaque salt should be used in each case.

For a considerable period now I have been using a mixture of 4 oz. of porridge with 1 oz. of milk and a little brown sugar to which 2 oz. of barium sulphate are added. This mixture is better than a suspension of the salt in water, as it approximates much more closely to a normal breakfast and excites a normal secretion of gastric juice; consequently peristalsis is more active, and the pyloric mechanism comes into play in a way which never occurs when a suspension in water is drunk. An advantage it has over most other mixtures which have been used is that it makes an admirable suspension of the salt which does not tend to form a sediment; consequently the whole outline and the upper limit of the gastric contents can be clearly seen. I have on previous occasions given reasons for preferring bismuth oxychloride to bismuth carbonate, but I now invariably use barium sulphate, as its price is one-twentieth of that of bismuth salts, and it has the advantage in cases of diarrhoea of not combining with sulphuretted hydrogen, and consequently of not tending to stop the condition which it is desired to investigate.

2. *The Stomach:* (*a*) *Position.*—The normal stomach varies considerably in position, but I suggest as a standard for the diagnosis of gastroptosis that normally the lesser curvature should not be situated below the umbilicus in the vertical position, and that the greater curvature should not be below the umbilicus in the horizontal position. It is important to distinguish between the two varieties of gastroptosis, one of which is present in the vertical position alone, the other in the horizontal position as well. A possible criticism of this standard is the variation in the position of the umbilicus. It is, however, an exceedingly convenient landmark, and its variations are only considerable in stout people with pendulous abdomens. In such cases the fallacy can be avoided to a great extent by hanging a penny by a long piece of strapping, which is fixed at the ensiform cartilage, and arranging it so as to be over the umbilicus in the recumbent position. When the upright position is assumed, the umbilicus may drop an inch or more, but as the penny is suspended from the fixed ensiform cartilage its position can be regarded as that of the normal umbilicus.

(*b*) *Tone.*—The tone of the stomach can be estimated by the position of the upper level of the gastric contents, and under certain conditions by the position of the greater curvature. It is extremely important not to mistake the upper level of the barium for the upper level of the gastric contents in cases of hypersecretion, in which the stomach already contains fluid and the porridge falls to the bottom, its upper level assuming an independent horizontal line. In cases of doubt the patient should be told to shake himself, when the splashing of the clear fluid under the gas in the fundus can be seen. I suggest that if the upper level of the gastric contents is less than 1 in. above the umbilicus atony should be diagnosed. The position of the greater curvature is, of course, no help to the diagnosis of atony, as extreme gastroptosis may be present with normal tone and extreme atony with very little ptosis. Hypertonus, however, which occurs most constantly in association with duodenal ulcer, can best be recognized by the position of the greater curvature. I should suggest calling a stomach hypertonic if the greater curvature is more than 1 in. above the umbilicus in the erect position.

(*c*) *Size.*—The size of the stomach needs no separate determination, as it depends entirely upon the tone. When the stomach is hypertonic it is also small, when it is atonic it is also dilated. The ordinary division into atonic dilatation and organic dilatation is inaccurate, as the latter is simply due to the tone of the stomach giving way as a result of increasing pyloric obstruction, and is therefore as much "atonic" as that which occurs independently of organic disease. The correct classification is dilatation due to primary atony, and dilatation due to atony resulting from obstruction.

(*d*) *Stasis.*—In order to determine the presence of gastric stasis, it is essential that the patient should be examined in the morning before any food has been taken, and that nothing should be eaten or drunk until after the second examination, which should be made six hours later than the first. In cases of suspected pyloric ulcer the stomach should be examined in the vertical position before any food is taken, as continuous hypersecretion of gastric juice occurs, which manifests itself by the presence of fluid recognizable with the *x* rays by the splashing visible under the gas of the fundus before breakfast. The whole of the standard meal should leave the stomach in less than six hours if the patient is up. A common fallacy, especially in hospitals, is to allow the patient to remain in bed between the two examinations, in spite of the fact that the stomach often empties itself twice as rapidly in the recumbent as in the vertical position. If more than a trace of the meal is still present in the stomach after six hours have elapsed stasis can be diagnosed. If peristalsis is observed to be weak, stasis does not necessarily indicate obstruction, and a second examination should be made in order to exclude the possibility that it was purely psychical in origin and due to fright or the unpleasant taste of the meal. When, however, peristalsis is active and stasis is present, obstruction can be diagnosed. If it is associated with gastroptosis the latter may be the cause of the stasis by causing obstruction at the fixed part of the duodenum, and a second examination should be made with the patient lying on his right side. If no more than a trace of the meal is left in the stomach after four hours, it can be presumed that the stasis was due to ptosis. When reversed peristalsis is seen at the first examination, when the upper level of the gastric contents does not reach the lesser curvature, or when such a degree of dilatation of the pyloric end of the stomach is present that in the horizontal position the pylorus is more than an inch to the left of the extreme right hand limit of the stomach, organic pyloric obstruction can be diagnosed even in the absence of a second examination after six hours.

3. *Iliac Stasis.*—About three and a half hours are required for the gastric contents to reach the end of the ileum, and a delay of an additional hour and a half is common in normal individuals before the chyme passes into the caecum. It is clear, therefore, that iliac stasis should not be diagnosed unless five hours have elapsed since the last trace of the meal left the stomach. As it is difficult for the patient to go for more than six hours without taking food, it is practically impossible to

diagnose iliac stasis in a single day unless the stomach empties itself with normal rapidity, except in the comparatively rare cases in which no barium at all has entered the caecum at the end of six hours. If, for example, the stomach is empty in three hours and no more food is taken until six hours after the barium meal, and a considerable quantity of barium-containing chyme is still in the end of the ileum eight hours after the meal, iliac stasis can be diagnosed. In the majority of cases, however, iliac stasis can only be diagnosed with certainty if some barium is present in the ileum twenty-four hours after the meal.

4. *Colon:* (*a*) *Position.*—The position of the colon varies greatly in normal individuals. The caecum and transverse colon are so often pelvic organs and the hepatic flexure so often drops into the right iliac fossa, that it is very difficult to say in any individual case that the ptosis which may be present is pathological. I have seen the caecum drop into the true pelvis in such a large proportion of healthy individuals with no stasis and no abdominal symptoms that I feel convinced that the occurrence should not be regarded as pathological. It is, however, uncommon to find a pelvic caecum in the horizontal position in healthy individuals. My own opinion is that one should state where the caecum, hepatic flexure, and transverse colon are situated in the vertical and horizontal positions, but add that the presence of the caecum and transverse colon in the pelvis and of the hepatic flexure in the right iliac fossa is not necessarily pathological. In some cases the splenic flexure drops with the rest of the colon when the vertical position is assumed. In my experience ptosis of the colon only leads to definite kinking when the splenic flexure retains its normal attachments to the diaphragm. If, therefore, the transverse colon falls into the true pelvis and the splenic flexure remains attached to the diaphragm, the condition can generally be regarded as pathological and likely to give rise to symptoms.

(*b*) *Stasis.*—Some recent investigations which I have carried out with Mr. Alan Newton of Melbourne have confirmed the opinion first expressed by Holzknecht that the large movement, which he and subsequently Barclay saw in the colon, is the normal process by which the faeces pass along the large intestine. I have now seen it on six occasions, and we have in addition frequently obtained evidence of its occurrence without actually seeing it. It occurs generally at the end of a meal or immediately afterwards, and it is undoubtedly identical in character with the movements which I had previously described as occurring in defaecation. As, therefore, the faeces may normally be carried in the course of a few seconds from one end of the colon to the other, it is clear that slight discrepancies from the average time at which different parts of the bowel are reached are of small importance. It is, indeed, exceedingly difficult to make any definite statement as to what are the normal limits in the *x*-ray examination of the colon. I believe, however, that the following varieties of stasis can be definitely separated.

1. If the greater part of the barium is still in the caecum and ascending colon at the end of twenty-four hours, stasis in this situation can probably be diagnosed; if the greater part is still present after forty-eight hours it can be diagnosed with certainty, even if a small quantity of faeces has been carried by a big movement to the distal part of the colon or the rectum, from which it may actually have been expelled.

2. If at the end of twenty-four hours nothing has passed beyond the splenic flexure, or if at the end of forty-eight hours the greater part of the barium is still in the transverse colon, the caecum and ascending colon being nearly empty and little or nothing having passed beyond the splenic flexure to the rectum, stasis at the splenic flexure may be diagnosed.

3. If at the end of twenty-four hours the greater part of the barium has collected in the rectum and in spite of this no desire to open the bowels is felt, dyschezia can be diagnosed.

4. If stasis in the caecum and ascending colon, as in Class 1, is associated with delay in the transverse colon, as in Class 2, and perhaps also in the descending colon, general colonic stasis can be diagnosed.

Whenever the delay is confined to a single point it is essential to give a barium enema. If some of a pint and a half of the fluid, injected at a pressure of not more than a foot and a half, does not reach the caecum in five minutes with the help, if necessary, of massage and change in position, and if the point of delay corresponds with that at which delay took place in the previous examination, an organic obstruction is probably present.

DISCUSSION.

Dr. C. F. Bailey (Brighton) had no doubt that a great many of the points which Dr. Hertz had put forward so strongly would fail to secure the agreement of a good many workers to a greater or smaller extent, but still it was a great advantage to have a definite issue set forward.

Dr. W. Ironside Bruce (London) had often thought lately that radiographers incurred a great responsibility in view of the large number of people who were subjected to operations, and whose abdomens were opened on these diagnoses. He agreed with Dr. Hertz as to the quality and quantity of the meal. It was obvious that it should be a standard meal, a palatable one, and a somewhat reasonable one. If large quantities of cold bismuth emulsion were given the stomach itself might reject the food simply because it was so unpalatable. Dr. Hertz, he understood, did not insist on his own particular meal, and it was open to them to use bismuth oxychloride, bismuth carbonate, etc., but it was important that there should be a standard. The speaker had been in the habit, in order to keep the food as even as possible, of advising that the patient should have an ordinary meal after the bismuth breakfast. He had done so because he believed that it was necessary to keep the condition normal. The patient would want a meal, and undue abstinence might make a difference in the action of the stomach in respect to the bismuth. Dr. Hertz had said that iliac stasis should not be diagnosed unless five hours had elapsed since the last trace of the meal left the stomach. The speaker believed that if complete iliac stasis were observed eight hours after the food was given under the conditions in which he gave it, the condition was mechanical, provided it was not a case of general gastroptosis. He confessed that he felt very nervous about the colon. One of his first cases was that of a young man who had undoubtedly at one time definite symptoms of obstruction. He had what appeared to be an acute attack of abdominal obstruction, and the food was seen to lie in the caecum and ascending colon for seventy-two hours. He believed, therefore, that he was safe in saying that there was some obstruction at the hepatic flexure, but when the surgeon opened it no sign of obstruction was found. Therefore he was always very careful in those cases in which the meal was retained for a long time in the form of segmented food without any sign of actual obstruction of the lumen.

Mr. J. Hall-Edwards (Birmingham) thought it highly important that a standard should be fixed. At the same time, it was one of the most difficult things to do. He found in a large hospital practice that it was almost impossible to get two patients to take the same quantity of meal. Personally he preferred to use bismuth carbonate, because, scientifically, he thought it the best substance. To use a material of such high specific gravity as barium sulphate was unscientific. Consistently with the proper density for the purpose, the substance should be as light as possible. Just as a matter of interest, he had had the meal not only weighed but measured. He gave 14 oz. of porridge and 3 oz. of bismuth carbonate. This weighed 17 oz. altogether, and on pouring it into a pint measure it was found to measure 15 fluid ounces. The question of weight had a good deal to do with the distortion of the stomach under certain conditions, and he thought it possible that a very heavy substance like barium sulphate might distort the stomach more than the carbonate. The ordinary meal given to a hospital patient consisted of:

Water	6 oz.
Pudding	6 ,,
Bread	2 ,,
Meat	4 ,,
Cabbage	4 ,,
Potatoes	4 ,,

The weight of this was 26 oz., and it measured 28 fluid oz. Any meal they were in the habit of giving thus fell far short of the standard meal as prescribed in hospital practice for filling the stomach, and it had been held that a proper diagnosis of the shape and position of the stomach could not be made unless the stomach was as full as it could be held without making the patient sick. In markedly dropped stomachs it was impossible to fill the stomach up under any circumstances unless an enormous amount of food were given. The great difficulty in standardizing the meal consisted in getting certain patients to take an amount as large as others. As to the position of the stomach and other organs, he thought that considerable harm was done to radiology by too definite statements which were made in the medical journals. The practice of stereoscopic radiography was of great value in the large intestinal kinks. It was almost impossible to make a diagnosis without this proceeding. He knew that many medical men palpated the abdomen during the meal and obtained thereby results which differed very considerably from time to time. The stereoscopic image gave a far better idea of the true position than palpations or any other method of examination. In a recent visit to Berlin he saw several photographs of this kind, and obtained evidence as to the value of the method. In conclusion, he would like to hear from Dr. Hertz why he chose a substance of great specific gravity for the meal in preference to one of low specific gravity. During the last two years he had registered upon his notes five patients sent to him by surgeons, who complained of not having had a moment's rest after a meal for a very long time. In several instances the administration of 5 oz. of bismuth carbonate had cured them. One gentleman, of whom he took a series of radiographs, was sent back to him in order that a further examination might be made. About three weeks afterwards this gentleman asked him for the name of the chemist from whom this special bismuth was obtained, and said that he had given up surgeons, and was going to prescribe 1 oz. of bismuth carbonate for himself three times a week for the rest of his life!

Dr. REGINALD MORTON (London) said that in the matter of standardizing the bismuth meal radiographers had been very lax. Some gave it semi-solid, others more or less solid. All this was grossly unscientific. For his own part he had given a dose made up either on a basis of bread and milk or of porridge, and had kept its bulk at about ½ lb. or 10 oz. This was not too small a meal for adequate observation, and not too large for the majority of people to take. A meal measuring 10 oz. contained 2 oz. of bismuth or barium salt. He could not see any material difference between bismuth and barium in their action on peristalsis. It would be very difficult, however, to arrive at any certainty with regard to that, as it would be necessary to subject the same patients to the same conditions repeatedly before positive information was forthcoming. As to distortion from the dropping of the stomach by the weight of the meal, if a meal were made up in the form he had mentioned and only 2 oz. of the salt were mixed in, the total increase in the specific gravity would not be considerable. The meal after the mixture of the bismuth would certainly be of a higher specific gravity than before, but this would count for a great deal more when the bismuth was given otherwise than intimately mixed with the bread and milk or porridge, as it would tend to drop down to the bottom of the stomach and give distortion. Mixed up with the bread and milk, however, it made a most perfect suspension.

Dr. F. HERNAMAN-JOHNSON (Darlington) remarked that a good deal had been said about the composition of the meal, but not anything about the psychical condition of the patients. Many of the patients had the idea that they were going to be hurt, and, moreover, the surroundings were somewhat awe-inspiring to the average person who had had nothing to do with radiographic matters. He had had a case in which there was thought to be atony of the stomach after a bismuth meal, but at the end of a quarter of an hour or so, when the patient found that nothing had been done to hurt her, the stomach returned to the normal J shape. The speaker protested against the tendency of some surgeons to open the abdomen for too little reason.

Dr. G. E. BOWKER (Bath) said that they were all feeling that they wanted a standard laid down for them to work with. If they gave 4 oz. of bismuth and the radiographs were taken or the screening done, the surgeon would ask why they gave 4 oz. when another radiographer gave only 2 oz. As this matter had been brought foward by Dr. Hertz, he hoped sincerely that it would not end in talk. He suggested that the Council of the Electro-therapeutic Section of the Royal Society of Medicine should collect evidence, and that each member of the Council should put forward the formula he usually employed in giving the bismuth meal. The Council might then express some preference as a Council, and there would be some chance of a standard which would furnish uniform observations.

Dr. HERTZ, in reply, said that it might be possible for either the Council of the Electro-therapeutic Section of the Royal Society of Medicine, or perhaps even that meeting itself, to form a committee which should obtain and discuss evidence and furnish a report on a standard meal for *x*-ray examination. The report might then be adopted in general practice by all who could bring themselves to do so. The committee might consist, say, of six members, and all who were interested in *x*-ray work should be asked simply to send in a report of what meal they used and why they used it, and thus they would arrive at definite conclusions. Mr. Hall-Edwards had said that he used bismuth carbonate because he considered it the most scientific method. He (the speaker) gave up bismuth carbonate because he considered it the least scientific. The first essential was that a salt should be chosen which interfered with the normal activity of the alimentary canal as little as possible. There were two chemical substances in the alimentary canal which might combine with certain salts, and the first of these was hydrochloric acid. It was known quite definitely from physiological and *x*-ray experience that peristalsis depended to some extent on hydrochloric acid, and the pylorus depended almost entirely on the free hydrochloric acid present in the stomach on the one hand and the duodenum on the other. Two ounces of bismuth carbonate neutralized every trace of hydrochloric acid in the stomach. The speaker some time ago carried out an experiment in which three or four patients were given a meal of milk and porridge containing 2 oz. of bismuth oxychloride on one day, and on another day the same amount, similarly administered, of bismuth carbonate. He then got his house-physician, who had no idea which meal had been given, to look for the more active peristalsis, and in each case it turned out that the peristalsis was more active after the oxychloride than after the carbonate. The absence of hydrochloric acid did interfere with the activity of the peristalsis, and that was why one should not use carbonate. The oxychloride was quite inert in the stomach. In hospitals the financial consideration had usually to be taken into account, and at Guy's, by the substitution of barium sulphate for bismuth oxychloride, they would save something like £50 per annum. Unless the bismuth oxychloride proved considerably better, he thought the financial consideration made it right to substitute barium sulphate. Moreover, barium sulphate had one little advantage. Bismuth salts stopped diarrhoea, neutralizing the sulphuretted hydrogen, which was often present in excess in that condition. But barium sulphate did not combine with sulphuretted hydrogen, and therefore it had no effect on these cases. It was a small point, but one of some little importance. In further reply to Mr. Hall-Edwards, he himself would not have thought that 2 oz. of barium sulphate evenly distributed throughout the whole of the porridge or bread and milk would have produced any distortion different from that of 2 oz. of bismuth carbonate similarly distributed throughout the whole meal. The matter was really one of physics; but with a proper suspension there could scarcely be any great difference, the weight remaining the same. Mr. Hall-Edwards had given some figures relating to the weight of the meal; and he, too, had had his meal weighed, and his (the speaker's) figures were very much smaller than those of Mr. Hall-Edwards. That did not mean that either of their results was necessarily the better, but simply that they were not comparable. He would be perfectly ready, however, to adopt a larger

amount than he had been in the habit of using if this were generally agreed upon. One advantage of the smaller amount was its more general applicability, for it was difficult to induce some patients to take a large quantity. It was a good thing to have a standard amount which every patient could take. The meal could never be really very palatable, and some total amount like 6 oz. would be better than 14 oz., but that was a matter for detailed discussion later on. The reason why he suggested porridge rather than bread and milk was that in his experience porridge gave a much better suspension. He used bread and milk at first, but he now proferred porridge. With regard to gastric and iliac stasis and the procedure of giving an ordinary meal after the barium meal, he considered this to be a point of the greatest importance. If one examined a patient at 11 in the morning and the patient had an ordinary meal at 1 o'clock, the stomach might be half empty at the latter hour, but the last trace of barium would not leave until the whole of the meal had passed. If tea, again, were taken at 4 o'clock the meal would be mixed up with the remaining barium, and in that way the delay would be increased. The same thing applied to iliac stasis. So long as there was barium in the stomach there would be some barium in the ileum, and one could not diagnose iliac stasis until five hours had elapsed since the last trace of the meal left the stomach. With regard to the psychical condition of the patient, Dr. Bruce had brought this question up at a previous discussion in connexion with the rate of evacuation of the stomach and the condition of stasis. Perhaps it also furnished an argument for doing a second examination to confirm the first before advising any operation.

At the close of the morning's proceedings the following resolution was carried unanimously:

> That this meeting expresses the opinion that it is advisable that one definite type of standard meal for radiographic examination should be adopted by general consent, and asks the Electro-therapeutic Section of the Royal Society of Medicine to appoint a committee to obtain evidence with the view of suggesting this "official" standard meal.

THE PRESENT POSITION OF THE X-RAY DIAGNOSIS OF PULMONARY TUBERCULOSIS.

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So much has been written on the subject of the *x*-ray diagnosis of early pulmonary tuberculosis during the past year or two that an apology seems necessary for referring to it again, but the question is one of such extreme importance to the profession and the public, that I consider it the duty of all who have had any extensive experience in the matter to come forward and express their views. It is only by collecting all the available evidence that we can arrive at some near approach to the exact truth. Thanks to the kindness and courtesy of my colleagues at the West London Hospital, and I would particularly mention Dr. Arthur Saunders and Dr. Sydney Owen, I have been enabled to examine a large number of cases of all ages, but nearly all in the early stages of the disease. Though my views for the most part tend to corroborate those of other radiologists, I consider it none the less important that they should be recorded.

In connexion with the subject of this paper, probably the most important event of the past year was a discussion in the Section of Electro-therapeutics of the Royal Society of Medicine. It was my privilege and honour to occupy the chair during this discussion, which extended over two meetings, and might have been continued through a third did circumstances permit. The vastness of the subject and the lateness of the hour made any summing up of the evidence or comments of my own out of the question, for after all a president's first duty is to preside.

I have given much thought to the points raised in this discussion, especially to the criticisms from the clinical side, which are of the greatest value to us as radiologists, and this paper is in a sense based on the evidence then brought forward, to which as occasion arises I interpose my own views. I have listened to many papers and discussions on this subject since 1904, and it is very interesting to note the change of attitude of the profession towards our work in that period. In 1904 the rule was that the paper was received with a silence that could be felt. This was disappointing, but as we now know, our work was crude, and it did not command the respect of the clinician. During the first session of the newly-constituted Royal Society of Medicine in 1907 a full discussion of this subject took place, and while we did not carry everything before us it was quite evident that our ideas were being more or less seriously considered by the clinical school. Since that time the subject has gone steadily forward, and thanks to great improvements in apparatus, skill, and technique, the *x*-ray method has attained an established position in the diagnosis of early pulmonary tuberculosis, and it is not too much to hope that this position will be greatly strengthened in the future.

What the ultimate position will be in this matter it is impossible to predict, but at the present time, at least, we must be careful not to claim too much for the *x*-ray method; it must remain an adjunct to the clinical methods, not a substitute for them.

It has been stated that one objection to the more general adoption of an *x*-ray examination is that of expense. In view of the uncertainty of our results there might have been some justification for this in the past, but considering that at the present time something very like positive evidence can be obtained in most cases, this objection should not exist. What is the significance of a consultation fee if through it we get for the patient information that is of the very first importance to him? In most cases it will prove the greatest economy, not only to his pocket, but also to his physical well-being.

The radiologist in making an examination of a chest for pulmonary disease notes the following points, among others:

Opacity, local or general, and the effect of deep inspiration thereon.
Movements of the diaphragm and ribs.
The size and position of the heart.
The presence of enlarged bronchial glands with striae radiating therefrom.
The presence of tuberculous foci.

Opacity.

It is very generally agreed that the presence of more or less opaque areas in the lungs, which do not illuminate on deep inspiration, as well as the surrounding parts, is very suggestive of tuberculous infiltration. Opacities due to other causes either light up on inspiration or the history of the case makes the matter quite clear. This sign is one of great delicacy and can be observed only at the screen examination. The tube should not be "hard" and the illumination relatively feeble to get the best results. It frequently happens that a patient who is "out of sorts," and has been feeling unwell for some time, presents himself for consultation—probably with a view to getting a tonic. There is no rise of temperature, perhaps a slight cough at times, no haemorrhage, and no definite history or physical signs of chest trouble. The physician is not satisfied, however, and suggests an *x*-ray examination. In such circumstances it is not uncommon to find one or more opaque areas such as have been referred to, and if a good plate be obtained other evidence may be found to which reference will be made later. Frequently it is only a question of time for such a patient to develop more definite signs and symptoms of tuberculous invasion, unless he has been put under more favourable conditions from the first; in fact, it is safe to say that all such cases should be looked upon as tuberculous and treated accordingly. The arrest or cure of the disease will be more rapid, certain, and economical if taken in hand at this stage.

That many such cases as this are definitely tuberculous has been frequently proved by the tuberculin test, which, if not infallible, is in the main quite reliable.

The Diaphragm and Ribs.

Limitation of the movements of the diaphragm—Williams's sign—was at one time regarded as almost pathognomonic of tuberculosis. At the present time we are not disposed to go so far as this. More extensive observation and better appliances have taught us that while

it is an important presumptive sign, no more than this can be claimed for it. The movement may be limited, jerky, or "stammering"; but these departures from the normal may be present in conditions other than tubercle, and absent in cases that we know are tuberculous. Very much the same may be said about the movements of the ribs. Free movement is presumptive evidence against tuberculosis, and vice versa; but more than this cannot be claimed.

The Heart.

A large proportion of cases of tuberculous invasion of the lungs have a relatively small and centrally placed heart; it is so common that it must be more than a coincidence. The view most commonly held is that this small and rather feeble circulatory pump is an important predisposing factor, and the chest to which it belongs has a lower resisting power than normal. While the presence of this sign is no definite proof, it indicates the existence of a soil favourable to the lodgement of the tubercle bacillus.

Enlarged Bronchial Glands and Striae.

Since the meeting of this Section at Liverpool last year, when Dr. Jordan read his paper on peribronchial phthisis,[1] the real meaning of these shadows at the hilum of the lungs has come in for a great deal of discussion, and it is doubtful if yet there is complete agreement on this interesting question. That many children and practically all adults living in towns have them, and that most of the latter at least are not the subjects of pulmonary tuberculosis, sufficiently proves that their presence cannot be accepted as positive evidence of anything in particular, unless to uphold the view expressed by Sir R. Douglas Powell, that since the bronchial glands are the dust-bins of the broncho-pneumonic system and the lethal chambers of many bacilli, they undergo enlargement as a result of the irritating presence of these foreign materials.

Even under the most favourable conditions, life as we understand it is a constant battle between the resisting forces of the body and the invasion of pathogenic bacteria. Though we may come through unscathed so far as any active disease is concerned, all sooner or later have their bronchial glands enlarged, with more or less well-developed bands radiating therefrom. Consequently, all we can say with any certainty is that, while the chest showing enlarged glands and striae on one or both sides may be tuberculous, it is not necessarily so.

Up to this point the *x*-ray examination is carried out with the fluorescent screen, and some observers have stated that a photographic plate is not necessary. In my opinion, and particularly in the earlier cases, it is impossible to be quite sure of the diagnosis without the additional and most valuable evidence afforded by a radiographic plate of good quality. Such a plate must show fine details, as obviously there will be little else to show in the earliest stages of the disease. It is not the purpose of this paper to go into the technique necessary to produce a satisfactory plate, but it may be said that the most essential point is that the exposure must be short—never more than five seconds. So-called instantaneous exposures are even better, but not necessary if care is taken.

There is no difficulty in getting patients, even children, to stop breathing for from three to five seconds, as the preliminary screen examination gives them confidence. No intensifying screen is to be used—the best of them is not good enough to be of any use in this class of work.

In such a plate we will find corroboration of some of the points noted in the screen examination, and if we trace out the striae leading from the enlarged bronchial glands it will be found that they lead to finer ramifications.

These are thickened lymphatic vessels, and the shadows of those near the surface of the lung, but on different levels, make that very characteristic picture, commonly called "mottling," but which might be better termed a network, though even this leaves much to be desired.

Frequently one of these striae will be found to run through or end in a tiny round or oval shadow—a tuberculous focus. These small foci scattered through the network of fine striae are probably the most characteristic appearance we get, and the most positive evidence we have of tuberculous disease of the lungs. Owing to diffusion and secondary radiation it may be difficult to bring this out in the early stages of hilum tubercle, and in a case affecting a considerable thickness of the lung these foci will be found in all stages of blurring or "woolliness."

This latter appearance has been put forward as evidence of activity, and this may be true as far as it goes, but if we have two of these little shadows side by side—one of the "woolly" and the other with a relatively sharp outline—we would have to be sure they were the same distance from the plate before we could say with any certainty that one was more active than the other. If these foci become fibrosed and perhaps calcified, so that they become obsolete they will cast shadows much larger, and we may assume that a large focus casting a dense, well-defined shadow is innocuous. I have a plate showing two such shadows near the hilum and close together. One has a well-defined outline while the other is distinctly "woolly." I do not believe the latter to be more active than the former; they are both obsolete and inactive. During the screen examination, by slightly rotating the patient to the right and left, it was quite easy to see that the "woolly" one was about twice as far from the screen as the other, and thus could not throw so sharp a shadow. If we could have our *x*-ray tubes sharply focussed we might, to some extent, eliminate this blurring of the shadows of opacities some distance from the plate or screen, but in the present state of our knowledge we cannot correct this entirely, owing to the action of secondary radiations. The most we can do is to allow the latter to act for the shortest possible time by using rapid or instantaneous exposures. In my opinion the best test of the activity of an early tuberculous pulmonary lesion is the presence of these small "pinhead" shadows, and the more numerous they are the more active and extensive is the lesion. Paradoxical as it may seem, a case undergoing recovery will show these little shadows gradually getting larger and more dense from the fibrous encapsulation. To the ordinary observer an examination of the plates before and after shows the last stage worse than the first, but the experienced radiologist is not deceived.

In estimating the value of enlarged glands and striae as a sign of pulmonary tuberculosis we must be careful to discriminate between adults and children, and also particularly note the character of the shadow in adults. As we have seen, practically all town dwellers give these shadows, but in the average healthy individual they lack the vigour and density seen in a case of tuberculosis in the active stage. The distinction is a fine one, and not to be estimated too highly, but it is worth noting.

Then as regards children, we may say that the younger the subject the more we are justified in attributing enlarged bronchial glands and striae to the presence of tuberculous invasion of the lungs, unless the previous medical history provides an adequate explanation.

Time does not permit me to go into the very interesting question of the most common point of origin of the disease in the lungs, but the evidence so far goes to show that the seat of origin is more frequently in the hilum than was generally supposed. Dr. Bythell goes so far as to say that in children the hilum is invariably the seat of origin. I am not prepared to go so far as this, and I note that Rieder is also disinclined to take such an extreme view. Dr. Bythell's experience of this disease in children is a very extensive one, and his view may be quite correct. On the other hand, "invariably" is a very sweeping term, and it will surprise no one if further experience makes it advisable to modify this to a slight extent.

I have occasionally been asked if there was a definite radiographic picture positively indicating the presence of pulmonary tuberculosis in the early stages. I am convinced there is such a picture, but I do not say we should depend on that alone; the other links in the chain of evidence must be well represented. Also the absence of the fine striae and tuberculous foci is not proof of the absence of the disease, as the latter may be so slight as to give no definite image, or there may be some fault in the *x*-ray technique, which is necessarily complicated in character.

It is highly probable that improvements in skill and technique will reduce the minimum amount of change in the lung that can be detected, but at the present

time it may be claimed that a careful *x*-ray examination will very often anticipate an independent clinical diagnosis.

In a paper by Dr. Sydney Owen and myself we have shown, to our own satisfaction at least, that there is a definite group of cases where the clinical signs and symptoms are so slight as to make any positive diagnosis out of the question, and yet in a large proportion of these an *x*-ray examination gave a positive, and in others at least a tentative diagnosis of tuberculosis. I am sure nothing more is needed to justify the inclusion of the *x*-ray method in the routine examination of all suspected cases.

And not only can the *x*-ray method frequently anticipate a clinical diagnosis, but it is found almost invariably that where a clinical diagnosis has been made, even by those possessing the highest degree of skill, the *x*-ray examination will show a more extensive distribution of the disease than was supposed.

The *x*-ray method has thus established for itself a very substantial claim for recognition as the most valuable one at our disposal for the examination of pulmonary tuberculosis, and its value will increase with its more extended and general use. It only remains for the profession and the public to demand it in all doubtful cases at least.

REFERENCE.
[1] BRITISH MEDICAL JOURNAL, August 31st, 1912, p. 484.

ELECTRO-THERAPEUTICS IN NEURON LESIONS.

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EVERY lesion affecting the nervous system is a lesion of neurons (or nerve cells), for the nervous system consists of neurons and neurons alone. Like other cells, each neuron is an independent living unit, dependent for its existence on the integrity of its perikaryon; it has, however, to an extent superior to that of all other cells, the power to transmit states of excitement with extreme rapidity (otherwise "conduct nerve-impulses"), and finally it is able to act on other neurons in such a manner that similar states of excitement are generated in them.

> The edifice of the whole nervous system is reared upon two neurons, the afferent root-cell and the efferent root-cell. These form two pillars of a fundamental reflex arch. And on the junction between these two are superposed and functionally set, mediately or immediately, all the other neural arcs, even those of the cortex of the cerebrum itself. (Sherrington.)

In practice, electro-therapy has limited itself largely to lesions of *peripheral reflex arcs* (for example, complete or incomplete nerve divisions, nerve-root injuries, and the destructive results, more particularly to the perikarya, in affections of the cord, such as infantile paralysis, etc.). That is to say, it concerns itself almost wholly with the lower neurons—neurons forming the simplest reflex arcs. Such a simple reflex arc must at least consist of (1) the sensory peripheral neuron (protoneuron), connected with various ceptors for receiving stimuli; (2) the conductor, made up of intercalary or interposed neurons; and (3) the peripheral effector neuron, causing action in an effector organ—in this case voluntary muscle.

Electric stimulation of any portion of a reflex arc will act, according to the position of the stimulus, on the muscle fibre at the end of the arc, either through nerve conduction or through reflex-arc conduction. The stimulus through reflex-arc conduction is commonly ignored as not very important, but in my opinion it should be looked upon as a valuable—perhaps the more valuable—method of applying electric treatment.

Practical experience in electro-therapy has convinced the majority of electro-therapeutists that electric treatment, intelligently and scientifically carried out, is a real advantage in the treatment of these lower neuron lesions; on the other hand, it is no uncommon thing, among physicians or surgeons outside our speciality, to come in contact with scepticism as to its value and an absolute objection to its use.

May not this feeling of doubt or antagonism be a sort of relic—a result of our former limited knowledge of the physiology and histology of the nervous system? Take, for example, such a simple instance as a divided mixed nerve with primary suture; are we not prone to limit our thoughts to peripheral degenerating nerve fibrils, certain muscle paralyses, and particular losses of different sensibilities? And yet who is not aware that the severance of the nerve means so much more? It may be that only extero-ceptive stimuli are abolished, while proprio-ceptive are unaffected; our mind may recognize the important result that a "final common path" of various reflexes is blocked, but omits to take cognizance of the many "afferent arcs" interfered with. These afferent arcs may be parts of many other reflexes, and each reflex may by inhibition act on others or may incite new reflexes into action, and such action may be a simultaneous one or may be successive; and so on.

Sherrington, in his lectures on the integrative action of the nervous system, lays stress upon the infinite complexity of results which may be produced by a very simple stimulus—such a one as the electric stimulus he employed—a similar stimulus to that which we use in daily treatment. He does certainly remark that

> electric stimuli not being of common occurrence in nature, there has been no chance for adaptation to evolve in the organism receptors appropriate for such stimuli, and that electricity therefore never constitutes the *adequate* stimulus for any receptor, being an artificial form of stimulus.

Nevertheless, it is a very efficient excitor, particularly in regard to reflexes which react to nocuous agents (pain reflexes or damage-threatening reflexes).

To hark back to our example, the divided mixed nerve, what is the modern view, shortly summarized, of such a nerve's structure and functions? It is a bundle of processes of neurons, each neuron being kept in a state of health by the integrity of its perikaryon (or "neurosome"). The function of each neuron is the generation and conduction of nerve impulses; if, like any other cell, it is deprived of work—made functionless—then, becoming useless, it will dwindle and waste away. But every single neuron is in such intimate relations with so many other neurons by means of its widely branching processes that it is impossible to isolate it and deprive it of stimuli. Though there is no connexion between the adjacent processes of separate neurons they are everywhere contiguous, though not continuous. Every neuron can and does actuate, by means of mittors on its dendritic ramifications, innumerable ceptors of neighbouring neurons, through synapses betwixt mittors and ceptors. Therefore any stimulus affecting any neuron may have an action (usually a beneficial one, because it means work) on hosts of other neurons. Electric stimuli, being of the prepotent type of stimuli, the "noci-stimuli" must therefore be especially valuable forms of stimulation. Let us examine the conditions of such a divided mixed nerve undergoing repair. New axon processes of protoneurons are slowly groping their way down through empty neurolemma sheaths towards end-organs of varying type, and at the same time new processes of peripheral motor neurons are growing down to muscle fibres; when they reach these end-organs or muscle fibres, as the case may be, in a couple of years more or less after nerve suture, they will be the subject of nerve impulses whose effects we can appreciate subjectively or objectively. But during all those months how many "peripheral reflex arcs" are out of action, and all the neurons making them up—protoneurons, intercalary neurons, and the peripheral motor neurons terminating on muscle or gland cell—all affected by the loss of stimuli which the absence of ceptors severed from the protoneuron entails. But ends of growing nerve fibrils can be stimulated by noci-stimuli, electricity in especial. Excited by an electric stimulus of the ends of its fibrils, a protoneuron becomes the seat of a nerve impulse which, transmitted to mittors at the ends of its central extremities, excites an impulse or impulses in intercalary neurons situate in the central nervous system, these impulses again exciting nerve impulses in peripheral motor neurons. Although these motor neurons are cut off from muscle fibres, yet they are attempting to cause contraction, and thus they are kept alive

and functioning. Theoretically, then, the use of a rhythmic stimulus, like the waxing and waning weak faradic current, with its opportunity for rest between action, to keep all these neurons awake must be a right one. Where the ultimate result of the initial nerve impulse, when reflected back to the periphery, would be to produce some modification in the activity of muscle fibres (or gland cells), and the result does not ensue because muscle fibre or gland cell is separated from its motor neuron, it is reasonable to believe that the neurons which are working, or attempting to work, will maintain nutrition or repair their damages quicker than if they were allowed to remain idle.

Then there is the fact that, although it is possible to damage (and often to damage very considerably) a "reflex arc" by accident or disease, yet it is practically impossible to destroy it absolutely. We may thus often be able to stimulate electrically proprio-ceptors to a marked degree, although we cannot affect extero-ceptors, because the nerve fibrils to the end-organs in muscles and tendons, etc.—the carriers of "deep" sensibility—may be intact, and their associated or other intercalary neurons may be able to stimulate the neurons which conduct impulses to the vessels supplying the part with blood; thus stimulation may (and does) lead to improvement in the nutrition of a part, and expedites a return to its normal.

Sherrington states that in all normal "actions, reflex or voluntary, especially in the limb regions, the centrifugal discharge to the muscles takes place through scattered motor fibres contained in several roots, even when the action provoked and the movements effected are weak." The severance of a single "motor root produces, not the complete loss of any one particular movement, but a weakened condition of many movements."

Dr. Lewis Jones, too, quotes another statement showing how scattered is the position of the nerve cells sending motor fibres to any one skeletal muscle, extending as it does through the length of three whole segments of the spinal cord as a continuous columnar group and intermingled at each transverse level of the cord with cells innervating many other muscles. He quotes him to support the view he maintained thirty years ago (which view has obtained confirmation year by year since), that prolonged treatment by electrical methods in nearly all cases of infantile paralysis will do good. He particularly points out the importance of the reflex mode of stimulation and argues the rational procedure of peripheral electric sensory excitation of a limb in infantile paralysis or of the face in a pure motor paralysis, like that of the facial nerve.

There is a period in the recovery of a mixed nerve after section and suture, early in Sherren's "third" stage, when restoration of sensibility to light touch and minor degrees of temperature coexists with hyperprotopathic sensibility, the power of accurate localization not yet having returned; it is associated, I have noticed, with apparently greater clumsiness than is present in the earlier stages. The patient, if the lesion is of the ulnar nerve, for example, frequently drops things should his attention wander, although, if his attention is active, his powers of delicate and complicated movements have become much better. This is apparently a result of antagonistic reflexes; the *tactual* reflex, which would result in maintaining the grasp, being overcome, unless reinforced by attention, by the *noci-reflex* set up by hyperprotopathic sensibility, the tendency of the *noci-reflex* being to withdraw the skin of the hand from the painful stimulus. Now this condition is greatly relieved by the stimulus of the faradic bath, with intermissions in the current; the hyperprotopathic sensibility is relieved and the clumsiness disappears.

In conclusion, I am becoming more and more of opinion that, as a method of treatment, omitting cases where ionic medication is required, the constant current might be given up. For testing purposes it will probably still be required, although the condenser methods seem likely to supersede it. I consider that the waxing and waning alternating interrupted current, with periods of rest, is of great value, and I prefer one where the rises and falls of potential are sudden; it seems most comfortable when given by means of a warm bath. Periods of rest seem very important.

THE VALUE OF CONDENSER DISCHARGES IN TREATMENT.

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I HAVE already drawn attention to the advantages which may be gained from the use of condenser discharges for diagnostic purposes, and that subject is to be further illustrated this morning in a communication from Dr. Cumberbatch. My object to-day is to advocate the use of condensers for treatment as a substitute for induction-coil currents when muscles have to be stimulated. In the treatment of paralysis the tendency at present is to use strong currents and long applications, and this has been especially insisted on by Bergonié, whose paper on the subject appears to me to represent a very important advance in electro-therapeutic practice. Moreover, the treatment of obesity by setting up powerful muscular contractions in many groups of muscles calls for a method of stimulation which can excite vigorous muscular responses with a minimum of sensory effect.

Much has been done in the direction of improving the induction coil in this respect, for we have learnt that by reducing the duration of the individual waves composing the current the sensory effect of induction-coil discharges is reduced, whereas the motor effect remains unaltered.

The disagreeable effects of electrical currents upon the sensory nerves are ionic effects, and the greater the movement of ions the greater is the amount of cutaneous stimulation, and the nature of the ion employed to moisten the electrode is also a factor, as can be readily shown by experiment. When we test muscles with condenser discharges, one of the most striking features noticed is the comparative painlessness of the discharges as compared with their motor effect, and this difference becomes more evident as the capacity of the condenser is reduced. It is natural that this should be so, because a small condenser gives a short discharge, and a larger one a longer discharge. Condensers provide a means of obtaining waves of current which are shorter than those of any induction coil, and on trial upon patients we find that condensers are admirable for producing painless muscular contractions.

In the stimulation of muscle a rhythmic interrupter is a necessity when best results are needed, and as this requires motive power of some kind to drive it, the same motor can be used to operate a revolving commutator, so designed as to go through the cycle of charging the condenser from the main and then of discharging it through the patient. The pantostat, which is now becoming so common a source of current for medical uses, possesses a motor, and is therefore capable of the further duty of operating a condenser commutator as well as a rhythmic interrupter. I have on the table a motor arranged in this way and fitted with a condenser commutator and a rhythmic interrupter. Though the example before you is the first of its kind, and happens to be somewhat unnecessarily complicated, it fulfils the requirements very well, and it will no doubt be simplified very shortly.

As supplied by the makers it is provided with condensers of one and of two microfarads, very cleverly arranged in the cover, and is intended to be worked off the resistance apparatus of the pantostat at about 20 or 30 volts. I find, however, that it gives even better results with a smaller capacity at a higher voltage. When this is 100 volts, a condenser of a quarter or an eighth of a microfarad gives excellent painless muscular contractions, and I shall be glad to demonstrate this at the close of this morning's meeting.

THE USE OF CONDENSERS IN ELECTRICAL TESTING, AND SOME RESULTS.

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THE subject of this communication is the use of condenser discharges for muscle testing, and a comparison of the results obtained with those given by the older method in

which currents from coil and battery are used for the same purpose. During the past few months I have tested a number of patients by both methods, and I feel sure that the new method possesses the advantages claimed for it by Dr. Lewis Jones, to whom we are indebted for its introduction into this country, and for the construction of a much more complete form of apparatus.

The first statement that I should make, and with the most confidence, is that the new method produces little or no pain, and the observer is soon impressed by the quietness of children and the stillness of their limbs, in place of the cries and convulsive movements which accompany the stinging pain of the battery current when the old method is used. In its other features also, to which I will refer later, the new method constitutes a milestone in the progress of electrical diagnosis.

In vol. vi of the *Proceedings of the Royal Society of Medicine*, 1913 (Electro-therapeutic Section, pp. 49–59), will be found a paper by Dr. Lewis Jones on the use of condenser discharges in electrical testing, and there the principles of the method are set forth and the apparatus described.

The apparatus is quite simple. It comprises a battery of condensers, ten or twelve in number, packed away in a small box. The smallest has a capacity of 0.01 of a microfarad, the largest has a capacity of 3.00 microfarads. There are eight or ten of intermediate capacity. By means of a metronome any one can be alternately charged from the mains and discharged through the patient. Condensers of different capacities differ in this essential feature, the duration of their discharge through the body. The smallest condenser in the box, that of 0.01 microfarad capacity, takes only $\frac{1}{3000}$ second to discharge through the body, but many healthy muscles can respond to impulses of such brief duration. But as the muscle proceeds further and further along the downward path of degeneration so does it require impulses of longer and longer duration from condensers of larger and larger capacity to induce it to contract. We can thus get a *measure* of the degree of degeneration and express it in the report by a figure indicating either the duration of the discharge necessary to make the muscle contract or the capacity of the condenser which produced it.

In the old method the coil is supposed to give impulses of very short duration; but Dr. Lewis Jones has shown that the wave of discharge of most medical coils is $\frac{1}{100}$ second, and that even if the effective part of it is only half of this, its duration is too long to reveal the slighter degrees of degeneration. In the Du Bois-Reymond coils used in experimental physiology, the waves of discharge are probably still longer, and they are not the instantaneous stimuli as they are called in the article on the physiology of muscle in Schäfer's textbook. Then, on the other hand, the impulse given by the battery current is much longer than necessary, and causes unnecessary pain. Science is measurement, and in our condensers we know the duration of their discharges, and in place of the two forms of stimuli provided by the coil and battery, one being not short enough, and the other being too long, we have a wide range varying from the very brief to the comparatively slow. The method of testing is simple and short. The electrodes are placed in position, the box of condensers is connected to the mains, the metronome set working, while by simply moving a key over successive studs we find out the smallest condenser which will just cause the muscle to contract. No pain to the patient, no necessity to decide whether the contraction is strong or weak, brisk, or slow. Such decisions are often very difficult to make, especially if there is no healthy muscle on the opposite side to give the standard of normal. And some normal muscles contract less briskly than others, and the muscles of a cold limb will also contract less briskly than those of a warm limb.

I have tested a number of patients by both methods, so as to determine the condenser figures corresponding to the three usual types of muscle reaction hitherto recognized—namely, normal, partial R.D., complete R.D. First, with regard to muscles showing normal reactions. Healthy muscles will contract to discharges from the smallest, or nearly the smallest, condensers. Muscles which have a superficial situation, like those of the face and many of the limb muscles, will react with a condenser as small as 0.01 microfarad. Others, lying more deeply, require the next size larger, 0.02 microfarad. Muscles which are weakened or paralysed, yet by the old method give a reaction which is normal in type, require larger condensers, 0.02, 0.05, or 0.07. Thus we have a numerical measure of different degrees of the normal type reaction. To give an example: A patient complained of weakness and wasting of the muscles of his arms:

Deltoid	0.01
Biceps	0.02
Triceps	0.05
Forearm muscles (supplied by median nerve)	0.05
Forearm muscles (supplied by posterior interosseous nerve)	0.05
Intrinsics of hand (median)	0.07
Intrinsics of hand (ulnar)	0.05

It will be seen that all these muscles responded with small size condensers; they gave normal reactions when tested by the old method. The condensers indicated that some of the muscles have departed, to some small degree, from the normal. The case proved to be one of muscle weakness accompanying osteo-arthritis.

When a muscle has passed into the condition in which it gives, by the old method of testing, the first indication of degeneration—namely, a partial R.D.—it is found that larger condensers are required—0.10, 0.25, or 0.50. Here are the figures from a report on the leg muscles of a patient suffering from progressive muscular atrophy:

Tibialis anticus	0.25
Extensor communis digitorum	0.10
Peroneus longus	0.10
Peroneus brevis	0.25
Gastrocnemius	0.50

These muscles, when tested by the old method, gave, beyond doubt, a partial R.D. The diagnosis of this reaction—partial R.D.—when sought by the old method depends mainly upon the decision whether the rate of contraction is quick or slow, and this is often very difficult. In the condenser method all we look for is whether the muscle contracts or not, and what condenser is required. In the figures mentioned we get an indication of three degrees of partial R.D.

I have tested muscles which, beyond all doubt, gave a partial R.D. and yet responded to condensers smaller than those mentioned above, condensers to which we would expect only muscles with normal or weak normal reactions to respond. I have seen muscles with partial R.D. responding to condensers of 0.07, 0.05, and, in one case, 0.02 microfarad. It is difficult to say yet what this means. Perhaps there is another type of reaction between weak normal and partial R.D.

In cases of complete R.D. the condenser method gives much information, for it shows that muscles with such a reaction vary within wide limits, and indicates the degree to which they have passed. Cases of paralysis, due to peripheral neuritis, progressive muscular atrophy, poliomyelitis and nerve injury have been tested by both methods, and I have found that muscles which show the complete R.D. will not respond to condensers smaller than 0.50 microfarad. A few muscles gave a response with 0.25, but it was found, testing by the old method, that if the coil stimulus was made stronger, some contraction occurred, and therefore the reaction was really a partial R.D. Evidently between 0.25 and 0.50 we are passing the limit between partial and complete R.D. Proceeding on the downward path, some muscles require condensers of 1.00 microfarad, others 2.00, others 3.00. The torpor of the last muscles is such that they required an impulse lasting three hundred times longer than that required for a normal muscle. But it was soon found that many muscles had degenerated still further and required condensers larger than 3.00 microfarads. Larger condensers cannot be used because they produce disagreeable sensations. Dr. Lewis Jones has surmounted the difficulty by introducing into the circuit with the patient a high resistance, and this has the effect of still further lengthening the time of discharge, but it is necessary to charge the condensers to a higher voltage. This can be quite easily and quickly done. So if a muscle cannot respond with a condenser of 3.00 microfarads, the condensers are charged to 200 volts, an extra resistance is introduced into the discharging circuit, and the testing is continued, first with the 1.00 microfarad condenser, then with the 2.00 microfarads, then with the 3.00 microfarads. I have found

5,000 ohms a suitable resistance, and with this in the circuit the times of discharge of the last three condensers is increased, respectively, to $\frac{1}{10}$th, $\frac{1}{30}$th, and $\frac{1}{50}$th second. So far I have not found a muscle showing a complete R.D. that would not respond to a stimulus lasting $\frac{1}{100}$ second.

Here are some figures from the report on a few of the muscles of a patient suffering from neuritis.

Muscle.	*Condenser.*	*Duration of Discharge.*
Tibialis anticus...	1.00 ...	$\frac{1}{150}$ sec.
Extensor longus hallucis ...	3.00 ...	$\frac{1}{150}$ sec.
Extensor communis digitorum...	2.00 (with 5,000 ohms in circuit)	$\frac{1}{30}$ sec.

Thus we see that some muscles with complete R.D. have just reached the stage in which this reaction is given, requiring a stimulus lasting $\frac{1}{150}$ sec.; others have proceeded far, and require one lasting $\frac{1}{30}$ sec., or forty times longer.

Since we are able by the condenser method to express the degree of degeneration in figures, we can get a clearer idea of the *progress* of a paralysed muscle. Thus one of the muscles of the patient just mentioned, when first tested, required a condenser of 0.02 microfarad; two months later, 0.05; three months later, 0.10.

Another patient, with progressive muscular atrophy of his leg muscles, was tested twice with a week's interval in between. On the second occasion it was found that each of his tested muscles required a condenser one size larger. This suggested that his muscles had retrogressed during the short interval of one week. The patient stated that on the first occasion he had been tested after an electric bath, while on the second occasion he had not had his bath before the test. Accordingly the test was repeated after the bath, and it was found that many of the muscles responded with the same condensers as on the first occasion. The patient volunteered the statement that his muscles always felt stronger for some time after the bath. The suggestion that the lowering of the condenser figures indicated improvement of the condition of the muscles by the electric treatment will probably be criticized. I do not think that the lowering of the figures was due to diminished resistance of the body following soaking of the skin in the bath or to the entry of ions, because the skin was continuously soaked in salt solution during the testing, which lasted longer than the bath. And in the bath a sinusoidal current was used, not a direct current.

In conclusion, it may be said that many of the results shown by the condenser method are what we might expect, normal muscles responding to a stimulus of brief duration given by a small condenser, degenerate muscles requiring a longer stimulus given by a larger condenser. Yet there are many results which are obscure. Why is it that some muscles with partial R.D. require medium capacity condensers, others with the same reaction small capacity condensers such as are used for normal or nearly normal muscles? Why is it that some muscles with a less severe degree of degeneration, as indicated by condensers of 2.00 microfarads, are not under voluntary control, while others with a much higher degree of degeneration can be voluntarily contracted? The investigation of these questions by the condenser method, and the comparison of the results with those obtained by the older method, is likely to lead to further knowledge in electrical diagnosis.

DISCUSSION.

[The paper by Dr. Bailey and the two foregoing papers on condenser discharges were discussed together.]

The President, Dr. DEANE BUTCHER, said that Dr. Bailey had taken advantage of an accident to his own arm in order to make a study of neuron lesions at first hand. He had done well to remind them that, after all, electrical treatment was the treatment of neurons. His explanation of the cause why there were so many different treatments for the same disease, which had been made a reproach to electro-therapeutic art, seemed to him to be new and convincing, and his paper marked some progress in the theory of treatment. The importance of the communications by Dr. Lewis Jones and Dr. Cumberbatch could scarcely be exaggerated. If science was measurement, then for the first time they were in the position of having a really scientific indication of what they found in muscle testing, and an indication which was explainable to one another and to the whole world.

Dr. H. LEWIS JONES said the old notion of partial R.D. was clearly going to be split up into several different conditions. Hitherto more than one muscular state had been confounded under the title of "partial R.D." Possibly some of the muscles which, while preserving the response to the induction coil, responded with unusual sluggishness when tested by waves of greater length, had healthy and diseased fibres side by side, while others represented a degeneration to a certain degree of all the fibres of the muscle. Partial R.D. would ultimately be analysed into several components. A drawback to the use of the metronome as an apparatus for charging and discharging the condenser was the possibility of giving the patients an unexpected shock when the metronome was not in good order. This form of interrupter involved the use of three mercury cups and three wires, one wire always being in contact with the middle mercury cup and the two others dipping alternately. A little derangement, by which these two were in the mercury cups at once, would mean that the full charging voltage passed through the patient, which was always inadvisable, though not dangerous, providing there was a resistance, such as a low candle-power lamp, in the circuit. But in the case of the commutator no mishap of the kind could take place. He intended to work further upon the development of that particular commutator and to apply it, if possible, not only for treatment, but also for testing. An induction coil, driven by a dry cell or an accumulator or what not, lacked uniformity. It might happen in ten or fifteen minutes' treatment that the dry cell driving the coil got "tired" before the end of that time, and one could not tell how it was acting, save by asking the patient what it felt like, and that was unsatisfactory. In the case of the condensers and commutators one knew that the action was just the same at the end of the longest application as it was at the beginning. The effect was readily ascertained on taking a reading of the capacity and voltage.

Dr. REGINALD MORTON said that he had always been displeased with the unsatisfactory reports one had to send down regarding muscular degeneration. There was always a difficulty with those muscles which gave the confused reactions generally classed together under partial R.D. Some which were thought to have a chance of early recovery got worse in spite of treatment. The condenser offered a means of getting the report in terms of scientific measurement, and if it were possible to express the graduations of the reactions in figures they would arrive in the course of time at a report on the condition of a muscle which could be exactly understood in any part of the world. The conditions of a test might be absolutely indicated; and this was an enormous advance, because it brought the whole question of electrical testing within the purview of scientific work. He himself had shown in the Electro-therapeutic Section of the Royal Society of Medicine an instrument almost identical with the one brought before them that morning by Dr. Lewis Jones, save that he (the speaker) had not coupled it up with the use of the condenser. He had displaced the induction coil and used the Leduc commutator, and by that means again he was able to duplicate the conditions of the test at any time. The curve of the current was constant, and it was also coupled with a rhythmic interrupter, which he regarded as one of the most important things ever introduced in the treatment of paralysed muscles.

Dr. C. F. BAILEY said that the theoretical reasons for many of the points at which Dr. Lewis Jones had been hammering for thirty years were only now becoming apparent. The speaker had been very much interested in the condenser, and he believed that he was Dr. Lewis Jones's first patient to undergo this form of treatment. One interesting point was that after faradic contractility returned a muscle was thought to be more or less normal, but it was by no means normal by the condenser method.

He had been using the condenser method for testing at the Sussex County Hospital, and one interesting case which was recently cleared up by this method was that of a girl who was supposed to have progressive muscular atrophy. When she was tested by the condenser methods, however, the reactions to the condenser were normal, and on going into her history it became evident that it was really a case of neuromimesis or hysteria. The only trouble about the condenser was that up to the present it had not been possible to use it for treatment, but the apparatus of Dr. Lewis Jones was going to help them in this respect very much indeed. The speaker had been working up to the present with the metronome interrupter. He had never found any particular difficulty with that, but he had always been careful that the two needles did not go simultaneously into the mercury cups. It was an excellent method for testing, but not the sort of method one could use very well for treatment. Dr. Lewis Jones's commutator gave them the opportunity of using condensers for treatment. Some arrangement was needed by which the same instrument could be used for both purposes. The question of voluntary control as compared with faradic response was rather important. Although thoroughly good faradic contractility might be obtained, voluntary action due to will power might be lacking. In his own experience he had found very often good faradic contractility without response to voluntary impulse. Sometimes the faradic response came first, and sometimes the voluntary power of contraction, in the period of recovery. In the case of voluntary contraction a great deal more must be involved than an impulse coming down to contract these particular fibres. The sensory element must be mixed up with the voluntary action. In his personal experience, after ulnar nerve section he found the muscle which adducted the little finger contract perfectly by faradism when he could not make it contract by will power, whereas the other muscles were contracting to will power, and yet presented the same reactions to faradism as the particular muscle which contracted to faradism only.

Dr. E. P. Cumberbatch mentioned a case of neuritis in a boy of 10 years who had no control over his leg muscle, which nevertheless reacted to the smallest sized condenser. That contrasted with cases otherwise similar which required the very largest possible condenser to make them contract and yet had retained will power. There were several obscure points like this upon which inquiry would lead doubtless to further knowledge.

Dr. J. D. Harris (Exeter) drew attention to the size of the pads used for electrodes in Dr. Lewis Jones's instrument. It had always been an axiom in the teaching of nurses that if it was required to affect the deeper structures the larger pads must be used, and if the action required was superficial the smaller pads had their turn. Some of the ambiguous results recorded might have been due to the size of the pads. It appeared to him also that the wetting of the skin was a point of so much importance that in note-taking it should not be lost sight of.

Dr. F. Hernaman-Johnson (Darlington) thought that the medico-legal aspect of the case was worthy of some consideration. He had known more than one case brought before the courts in which the appellant had been unable to move his muscles voluntarily, although they had responded perfectly to faradism. Therefore, it was an important matter when Dr. Bailey and others said that one could have faradic reaction without control by the will. He had seen a number of cases go against people on this ground, the supposition being, in view of the faradic stimulation, that they were malingering. If it could be well established that faradic reactions were frequently obtained without control of the will it would alter the medico-legal aspect of the matter. Up to the present he had not been able definitely to contradict the view that because the muscle reacted faradically, although the man himself said that it was incapable of voluntary movement, therefore he should lose his compensation.

PATHOLOGICAL BLOOD PRESSURE AND ITS TREATMENT BY MODERN ELECTRO-THERAPEUTIC MEASURES.

By Francis Howard Humphris, M.D., F.R.C.P.Edin.,
President of the American Electro-Therapeutic Association.

The importance of recognizing a pathological blood pressure, and the advantages of reducing a tension unduly high, are to-day so generally conceded that it were superfluous to dilate upon them.

Ever since 1828, when Poiseulle introduced the V-shaped mercurial manometer, progressive and painstaking physicians have gradually brought the subject to its present state.

Although an altered blood pressure is only a symptom, and not a disease *per se*, it is a symptom of such importance that in some cases diagnosis and prognosis may be decided by it, and in others treatment may be modified by its variations, and, again, it may be in itself a symptom which urgently calls for relief.

"A man is as old as his arteries," and if it were only that an alteration in blood pressure gave a warning of advancing years, that fact alone would justify attention to the subject-matter of this paper. There are, however, many conditions resulting from, or concurrent with, an alteration in arterial tension that make the subject interesting as well as vital.

Increased arterial tension is, in all probability, responsible for the degeneration of the muscular walls of the arteries; in fact, nothing else can be conceived, if granted the pathologically increased tension, than that the persistently-contracted muscular structure of the arteries should suffer. Muscles in any part of the body degenerate under constant tension or contraction.

The importance of early recognition and treatment of the altered blood pressure is, therefore, obvious; if we can recognize the pressure before it has had time to cause the degeneration, then we may, by the various therapeutic measures, diminish that pressure and thus avoid the otherwise resultant degeneration.

In some cases, where the blood pressure is giving rise to nervous trouble, we can, I believe, by an approximation of the blood pressure more nearly to the normal, prevent those troubles from ending in permanent disease, and from crossing that border line between nervous disease and mental derangement to which they are surely but slowly progressing.

It may not be out of place to submit a list, modified from Leftwich, of some of the conditions which give rise to, or in which we may expect to find, a high blood pressure, or to which it may lead:

High Tension.

Arterio-sclerosis, especially where there is sclerosis of the splanchnic vessels, or of the aorta above the diaphragm, and also when associated with renal disease.
Angina pectoris.
Aortic aneurysm.
Chronic bronchitis.
Cirrhosis of kidney.
Cerebral tumour (first stage).
Chronic alcoholism.
Constipation.
Cerebral haemorrhage.
Cheyne-Stokes breathing (which, with high tension, is ominous).
Dilatation of aorta.
Emphysema.
Glycosuria and albuminuria.
Gout.
Hemicrania.
Hypertrophy of heart.
Hysterical seizure.
Lead poisoning and other toxaemias.
Malaria (cold stage).
Meningitis.
Migraine.
Neuralgia and other conditions of bodily and mental suffering.
Pregnancy.
Pleurisy, acute (first stage).
Uraemia.
All kidney affections except amyloid, suppurative, and tuberculous, except in terminal stages, when cardiac weakness causes a fall. Hypertension is absent, too, in some toxic nephritis, and in those cases caused by certain of the infectious diseases.
A too nitrogenous or too liquid diet.

Insanity with depression, including general paralysis of the insane. Also found in healthy but apathetic individuals.

Certain drugs may act in raising pressure (especially when injected): Extract of the fundibular part of the pituitary body, caffeine (preferably in the form of caffeine-sodium benzoate), strychnine and camphor, ergot, barium chloride, oxygen, nicotine, digitalis (of doubtful value).

Cold weather.

Descending galvanic current.

Treatment must be directed in all cases not only to the approximation of the blood pressure to the normal, but also to the ascertaining of the cause and its removal; but though the cause, be it dietetic, hygienic, toxic, or what not, is of prime importance, this is not the place to discuss it; it is mentioned to emphasize its significance.

Unless the abnormal tension be compensatory, efforts should be made to reduce the abnormality.

It is generally conceded that electrical currents have the power of modifying pathological arterial tension, and yet have little or no effect upon normal blood pressure (indeed, the same had been observed of the action of digitalis[1]). It has already been pointed out that in a slight degree the static bath will approximate to the normal either an increased or a decreased blood pressure, but for greater effects other currents must be employed. The precise way in which these act has not yet been fully determined, but this much may be taken for granted—that their action is upon the muscle fibres of the arteries, either upon these directly or indirectly through the vasomotor centres. In practice, if we wish to raise an abnormally low blood pressure, the static brush applied to the spine will be found fairly effectual. Static sparks in obstinate cases will produce a definite effect perhaps in a shorter time.

Much good may be done in case of high blood pressure when the increased tension is due to imperfect elastic recoil in the arteries (even with arterio-sclerosis) and increased peripheral resistance. When, however, there is a falling ventricular compensation it is wiser not to attempt to lower the peripheral resistance, for this resistance is an attempt on the part of nature to compensate for the failure of another compensation.

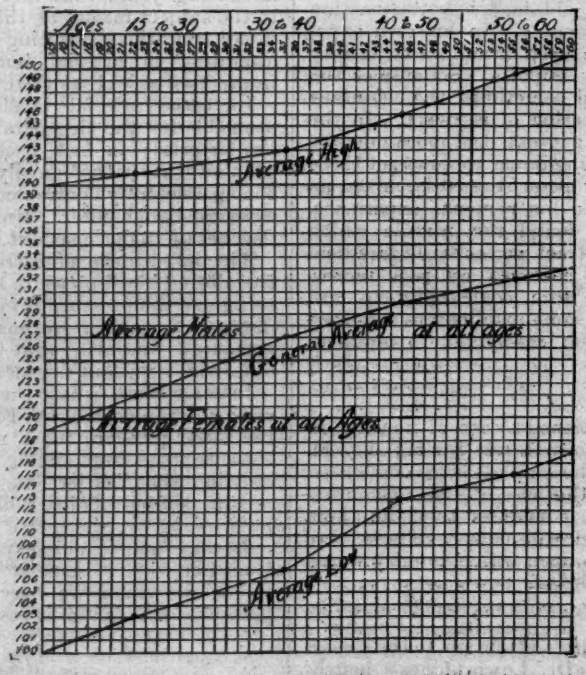


Chart showing normal limits of variation in systolic blood pressure.

In the former class the great majority of cases fall, at least those which come to the physician for treatment.

Electrically there is a simple means of determining whether or no it would be wise to attempt to lower the pressure—that is, whether we have simply imperfect elastic recoil with increased peripheral resistance, or whether we have broken cardiac compensation.

If a patient connected with a Tesla coil (arranged in a manner similar to that of the autocondensation couch) show any lowering of tension with a current of 20 to 50 milliampères, we may conclude that the peripheral resistance is of vasomotor origin, and should be left alone. If on the other hand no change is shown, then it may be taken that the case is suitable for electrical treatment.

Of course, it is superfluous to test every case in this manner; as a rule, it is easy to decide the class of case with which we have to deal.

Another method of differentiating is by means of amyl nitrite, but, as this paper confines itself to electricity, it would be out of place to go into details of this. Vasomotor insufficiency may also be detected when the sphygmomanometric reading is not proportionately higher in the standing than in the recumbent position.

Etiology.

Having generalized rather discursively before proceeding to the treatment, in order to get a clear picture of what is to be treated, let us look briefly at the etiology and symptoms.

Inherited tendency undoubtedly determines our general resistance, and arterial tissue is no exception to this rule. When Oliver Wendell Holmes was asked the secret of living to an advanced age, he replied that the secret consisted in choosing one's ancestors 100 years before being born; but the congenital form is happily rare, and more rarely still comes under the notice of the physician practising electro-therapy. Woley has shown in the accompanying chart what are generally accepted as the normal limits of variation in systolic blood pressure.

One word should be said about the normal maximum arterial tension in advancing years. It is usually taught that its rise with advancing years is physiological. True it is that as a man gets older his tension usually rises, but the fact is that his tension rises not on account of his years, but on account of something wrong in his health; and should not a man who has always lived a healthy, active, abstemious life have practically the same blood pressure at 60 as he had at 30? I think so. I think that in order to live to an advanced age one should have an estimation of one's blood pressure made every six months, and on the appearance of any tendency for the pressure to rise, the cause should be sought and removed, and unless the cause of the rise contra-indicate its reduction, the pressure should be reduced by the electrical method described later.

Sex.

It is generally admitted that men are more liable to hypertension than women, and when the list of its causation is complete it will be apparent why this should be so.

Occupation.

Any occupation entailing long muscular exercise and lifting heavy weights would seem to be especially provocative of hypertension. It is easy to understand this, for it can always be noted that the mercury will rise in the sphygmomanometer when the patient makes muscular effort, and any repetition of a temporary condition is apt to make it permanent.

Among the better class of patient worry is a frequent factor in raising the blood pressure, and prolonged mental strain can very well be traced as cause in financial magnates, stockbrokers, theatrical managers, etc.

Diet and Habits.

Alcohol and tobacco are convenient scapegoats on which to lay the blame for the condition of hypertension, but it must be left for pathologists and statisticians of a later day to judge. Evidence is too conflicting at present. The moderate use of either, or both, I have yet to see clearly established as a cause. Far more certain is it that rich and indigestible food—and especially a plethora of it—will cause a high blood pressure. This it may do in two ways, either directly, by frequently causing a temporary hypertension, a condition which eventually becomes permanent, or indirectly, by setting up an intestinal autointoxication, and the toxins thus liberated circulate in the blood and act as a chemical irritant.

Disease.

The diseases which may act as etiological factors have already been given. It may be noted that syphilis was

not included, and it was omitted for this reason, that in the majority of cases syphilis causes arterio-sclerosis, which in its turn gives rise to hypertension directly.

Signs and Symptoms.

High blood pressure, especially in the presclerotic state, may exist without any signs or symptoms, or with these so vague as to be barely recognizable by the patient; and may, indeed, be only discovered by the sphygmomanometric reading, or there may be certain functional symptoms, such as attacks of transient giddiness; headache, often intensified with the effort of mental concentration ("the sign of painful thought"); epistaxis; palpitation; slight oedema of the ankles; dyspepsia (stomach contents usually being subacid); insomnia or sleepiness, with a disinclination to work; irritability of temper; a tendency to tire easily after bodily or mental exercise; anxiety, or a sense of impending evil impossible to define.

Thus it will be seen that high blood pressure may evidence itself in a variety of ways, none of them pathognomonic; but they usually occur grouped together in such a way as to suggest collectively an altered blood pressure.

Here we have a condition in which electricity can accomplish much. Pathologically there may exist a process of degeneration, which we can check, and so prevent an arterio-sclerosis, and ward off such imminent consequences as apoplexy, kidney affections, and the myriad ills that follow in the train of an arterio-sclerotic condition.

With d'Arsonvalization, properly administered, much more can be accomplished than with most measures employed for treating hypertension. With it, except in the very advanced cases, a patient can be kept for years in a condition of safety, judicious attention, of course, being paid to general lines of alimentation and exercise.

The therapeutics of this current in lowering the blood pressure are twofold. One, the general or constitutional effect, as affecting metabolism; and the other, the local effect on the vasomotor system. The general effects of the current as influencing metabolism have been demonstrated both clinically and chemically. There is a gain in physical and mental strength and general improvement in the health, and the symptoms involved, be they insomnia, palpitation, or anxious fears, disappear. Chemically, there is an increase of solid in the urine, and this has also been found on analysis of the perspiration.

The improvement in general metabolism is probably largely due to the effect on the vasomotor system, whereby the arteries becoming relaxed, permit of a freer circulation of blood, and if this one fact be granted, then the beneficial effects of the current are easily explained. A subsidiary cause may be at work, and that is the thermic effect of the current. This is quite apparent to the patient, and it may be that this causes an increase of functional activity by which the elimination of the toxic materials is augmented.

The current of d'Arsonval, of which I have been speaking, consists fundamentally of a high potential current, conducted through a spark-gap, two condensers, and a copper solenoid. The particular arrangement that I have found most successful, and one capable of giving up 2,500 milliampères by the autocondensation method, may be thus described (see diagram):

The current is led from the direct main in the ordinary way into a 12-in. coil through a switchboard provided with a voltmeter and ampèremeter marked A and V. Also there is a knife cut-out and three resistances, one controlling the speed of the interrupter (2), and (3), the other two (diagrammatically represented as one), regulating the current strength. From the coil the current is led into a multiple spark-gap. This multiple spark-gap was designed by Dr. de Kraft, of New York.

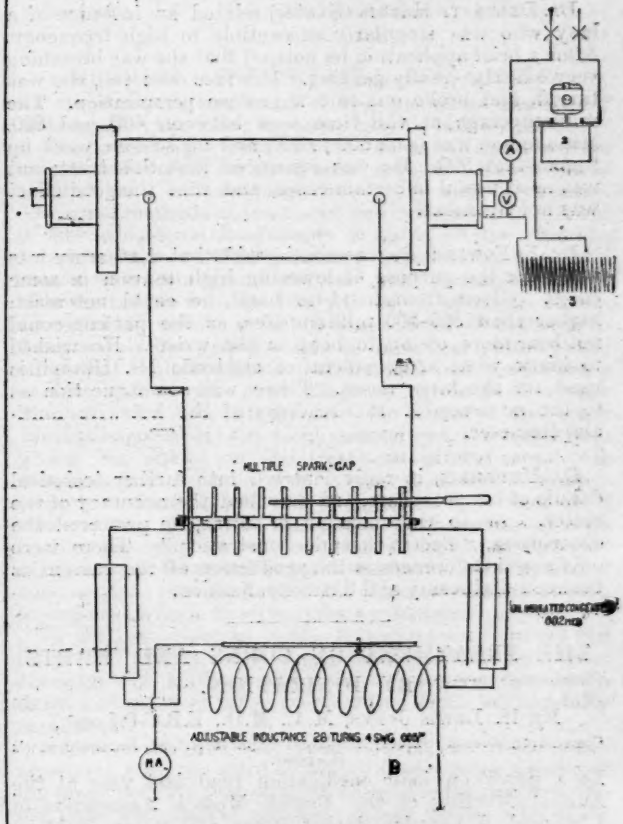
This is one of the simplest possible type. There are nine gaps, each measuring about 2 mm. The sparks are discharged unmuffled in the open air owing to the extreme shortness of the sparks and the multiplicity of them. The noise is much less than with the ordinary high-frequency spark. More recently I have been using a spark-gap which is noiseless, each of the small gaps being enclosed in a small receptacle.

The result from having a greater number of sparks in increasing the rate of oscillation to be obtained from the d'Arsonval spiral is astonishing.

The current from the spark-gap is led to two insulated condensers having a capacity of 0.002 microfarad. Thence it is led to an adjustable inductance resonator of copper wire (4 B.W.G.) having twenty-eight turns; from thence to one terminal attached to a sheet of metal, and to the other ending in a handle with a milliampèremeter (M.A.) in circuit.

The patient rests on a cushion, which is made of leather and filled with horsehair, measuring 22 in. wide and 72 in. long and 3 in. in thickness. He holds the handle in his hands, and should feel nothing except a sense of warmth, which is chiefly manifest in the wrists and up the arms as the current is slowly turned on and the milliampèremeter measures 1,000 milliampères or more.

A pleasant feeling of warmth and comfort pervades the body, and there is a tendency to somnolence. The treatment should be continued for fifteen minutes. The blood pressure should drop from 10 to 20 per cent.



Thus, the blood pressure which is 200 at the beginning of the treatment becomes 180 or 160. This will gradually rise till the following day, but not always, however, to its original point, when another treatment should be given; but after a time it will be observed that the pressure no longer rises between treatments. This may be taken to be the patient's normal pressure, and to keep it at this he will require to come for further treatments from time to time, as the varying conditions may cause an increase in the pressure.

Another form of treatment of hypertension, especially when this is associated with acute nephritis, is that by the electric light cabinet. The effect on the patient is almost always satisfactory—not only does he feel better, but with the increased elimination, both cause and effect are benefited.

Our lives depend upon the integrity of our hearts and blood vessels for the maintenance of activity and of life itself.

Comparatively young people grow old before their time. This is due often to the manner of life led. Whether excessive eating or drinking, or brain-work, or worry, which tend, as we have seen, to raise the blood pressure, pave the way for arterio-sclerosis, and are the means of hastening the patient to a premature grave. But if a mode of life conducive to a healthy old age be adopted, and proper

electrical treatment used when the pressure shows sign of rising, arteries may be kept elastic and youthful, and man will then be as young as his arteries.

REFERENCE.

[1] Czyhlarg, *Wien. klin. Rundschau*, April 15th, 1900.

DISCUSSION.

Dr. C. F. BAILEY (Brighton), speaking with regard to the influence of high frequency on arterial tension, mentioned that he had had one case, a diabetic, who was much improved by high-frequency treatment. He was interested to see whether the improvement was maintained; but it was not, and the man developed further trouble with his arteries. The speaker agreed with Dr. Humphris as to the necessity for a very high milliampèrage.

Dr. G. E. BOWKER (Bath) asked whether there was any distinction between high-frequency and diathermic treatment.

Dr. DELPRATT HARRIS (Exeter) related an instance of a lady who was singularly susceptible to high frequency. After a brief application he noticed that she was breathing very quickly—really panting. Her face was red, she was turgid, and broke out into a profuse perspiration. The milliampèrage at the time was between 400 and 800. His average was from 600 to 700, and he seldom went up higher than 900. He was convinced that this treatment was most useful in certain cases, and that the good effect was not transient.

Dr. F. FOWLER (Bournemouth) said that diathermy was useful for the purpose of lowering high tension in some cases. Given from hand to hand, he could not reach higher than 350–500 milliampères, as the patient could not bear more, owing to heat in the wrists. He wished to know what arrangement of electrode Dr. Humphris used for the large doses. There was some question as to the accuracy of the readings of the hot-wire milliampèremeter.

Dr. HUMPHRIS, in reply, entered into further technical details of his procedure, and defended the accuracy of the meter. As to the question of terms, he preferred the description, "electro-thermic penetration." There were only slight differences in the production of the current as between diathermy and d'Arsonvalization.

THE TREATMENT OF CORNS AND WARTS BY IONIZATION.

By H. LEWIS JONES, M.A., M.D., F.R.C.P.Lond.,

Consulting Medical Officer, Electrical Department, St. Bartholomew's Hospital.

IN a paper on ionic medication read last year at the Annual Meeting of the British Medical Association in Liverpool[1] I mentioned that corns and warts could be successfully treated by ionic methods, and expressed the opinion that corns were probably due to a microbic invasion, as is now considered to be the case with warts.

In my opinion, a corn is not merely a callosity caused by friction. Such callosities are common on the toes, but may remain harmless for years, neither growing nor becoming painful, whereas a genuine corn grows quickly, is tender, and often becomes painful. It is granted that a callosity due to friction is often a favourite site for the development of a corn.

The well-known treatment of corns by salicylic acid dissolved in collodion is said to act by virtue of a solvent action upon epidermal cells which is ascribed to salicylic acid. It is quite as likely that the action of the drug is an antiseptic action, and if so a watery solution of salicylic acid would probably be better, as such a solution would permit of an ionic action. Salicylic acid is not very soluble in water, but is much more soluble in water containing borax.

The treatment of corns has been rather neglected by medical men, but corns are none the less worthy of attention, as they cause a great deal of discomfort, and, one might add, are responsible for much bad temper. By ionization one can obtain very good results, and, in fact, one may claim to be able to cure them.

In addition to the treatment of a number of cases of ordinary corns, I have had three cases of severe corns of the sole of the foot, a variety which has the reputation of being unusually difficult to treat, but which yields satisfactorily to ionization with zinc. The procedure is simple, and consists in using first a zinc positive needle for the centres or foci of the corn, and following this up with ionization of the whole affected area with a zinc plate and a pad moistened in the usual way with a zinc salt. It is usual to prescribe a compress of zinc sulphate of a strength of 1 per cent. to be applied for some hours before the ionization, in order to have the horny epidermis imbibed with the solution beforehand, so far as this can be accomplished by simple diffusion.

Two of my cases were in women, and one in a man. All were completely freed of their corn in three or four weeks. It disappeared and left no trace. One was a lady at Oxford, captain of her hockey team, and much crippled in her movements before the treatment. I heard some months later that she remained quite well. The second lady was able to spend her holidays in mountaineering, and had no relapse. The third patient wrote me to say that he was out with his company of Territorials, and that the corn had completely gone.

There is not much to say in respect of warts. Transfixion with a zinc needle, with a milliampère of current for a minute, suffices for ordinary warts. If large, the transfixion is done twice, crucially. A little more current or a longer time may sometimes be desirable.

When warts are numerous and small and close together, ionization with a pad and zinc solution is quicker than needling of the individuals. For some warts magnesium answers very well, especially for the flat multiple warts of the backs of the hands and of the face.

The branching, coral-like warts of the chin and scalp are easily removed by the use of a zinc needle. Warts of the penis, which I have treated on one occasion, were satisfactorily removed by simple zinc ionization from a pad of zinc sulphate.

There seems to be some evidence that magnesium salts by the mouth exercise a specific influence upon warts. I have met with several cases in which small doses of magnesium sulphate caused multiple warts to crumble away and disappear in a very few days.

REFERENCE.

[1] BRITISH MEDICAL JOURNAL, August 31st, 1912, p. 485.

DISCUSSION.

Dr. E. P. CUMBERBATCH mentioned a St. Bartholomew's patient who had been completely cured of a thickened epidermis of the foot by the zinc ionization method.

Dr. HARWOOD NUTT said that at the Royal Hospital, Sheffield, he had treated a case of warts of the hands—the warts numbering fifty or sixty—by means of magnesium ions, and the excrescences cleared up after three applications. He also instanced the disappearance of a corn after two applications of salicylic ions.

Dr. IRONSIDE BRUCE said that he had made the warts disappear by means of cataphoresis. He had failed, however, to ascertain how the warts disappeared. Did they gradually become smaller, or did they drop away?

Dr. G. E. BOWKER (Bath) said that he had treated a few of these cases of warts with great success by means of magnesium ions. The warts in the one case covered more especially the tips of the fingers surrounding the nail, and there was one particularly large wart on the back of the hand. This would answer Dr. Ironside Bruce's question, for the wart dropped off, more or less like a ripe pear.

Dr. LEWIS JONES, in reply, said that very often the warts crumbled away. The patient said that they "rubbed off"; he gathered that it was usually piecemeal, but sometimes clearly they must come off *en masse*. He was very glad to hear any evidence that magnesium had been a success. A little while ago it was stated in France that the treatment of warts by magnesium was a treatment by systematic purgation. It was not so at all. No doubt magnesium acted well, although he used zinc in

preference, because his experience of zinc was greater, and he was rather more confident of its action. But it was clear that magnesium had a specific action when introduced ionically. Belot had said that he got just as much effect from the action of the constant current. But there was no constant current except a migration of ions, and, if the pad were wet with something else, he was simply using certain other ions. It had also been claimed that lime administered internally was a cure for warts, and one could not help thinking that what was wanted in the cure of warts was alkalinity, and that any alkaline material would cure them—lime, magnesia, soda, etc. It might be that the warts were due to a germ which grew in acid media and not in alkaline media, and that magnesium was one of many ions capable of eradicating it.

DEMONSTRATIONS.

I.

Dr. Thomas Lewis (London) described the method of taking simultaneous electro-cardiograms and heart-sound records, and gave a lantern demonstration of the results. The method, he said, was particularly valuable, because of the exact idea which it furnished of the time relations of heart sounds. His lantern slides included tracings of normal heart sounds and the common murmurs. Dealing with the character and time relations of the murmurs in mitral stenosis, he pointed out that, the heart's mechanism being normal, when the heart-rate was fast or the stenosis severe, the murmurs filled diastole, and when the heart-rate was slower and the grade of stenosis was less, they occupied presystole or presystole and early diastole. The presystolic murmur was not usually crescendo, the crescendo quality being given by the accentuation of the first sound. When the auricles fibrillated the murmurs filled diastole in the short cycles, or in all the cycles, when the heart-rate was fast. They lay in the early diastole of the long cycles, and were confined to early diastole when the heart-rate was very slow. Under the last-named circumstances the murmur might be separated from the preceding second sound by a short interval of silence. An isolated presystolic murmur was not found when the auricles fibrillated. The relation of the diastolic murmurs of mitral stenosis to the auricular contractions when heart-block was present showed that these murmurs were largely controlled by the positions of the auricular contractions. He added that the explanation of all the diastolic murmurs of mitral stenosis was to be found in a study of the differential pressures between the auricle and ventricle, and therefore in the velocity of flow through the mitral orifice.

At the close of the demonstration Dr. Lewis answered questions by Dr. C. F. Bailey and Dr. E. P. Cumberbatch relating to the cost of the electro-cardiograph apparatus, and to certain heart irregularities which were observed after operations.

II.

Dr. Franz Nagelschmidt (Berlin) brought forward a combined method of diathermy and electro-gymnastics in the treatment of nutritional disorders. He considered that dieting was frequently a failure. So long as the dieting continued the effect was good, but within a few weeks of the end of the dieting the disorders reasserted themselves. Dieting did not mean curing, but simply covering the symptoms. His own method, he claimed, involved not symptomatic treatment, but actual cure. He employed diathermy, or the electrical introduction of heat deeply within the tissues, by means of the Nagelschmidt diathermy apparatus, thus sparing the combustion of the body's reserve material, and at the same time stimulating the circulation and nutrition of the cells. Together with this he employed a method of electro-gymnastics of his own devising, which, he said, was free from the occasional disagreeable sensations of Bergonié's method, and at the same time stimulated a very active contraction of the muscles. The result was that while feeble persons gained in weight, over-stout persons tended to lose their surplus; the metabolism was thoroughly modified and righted, and the results remained steady after a long period of time. By combining a carefully regulated electro-gymnastic method with diathermy much good was done in heart troubles, and he mentioned that he had also found the electrical treatment of value in arthritic and other conditions.

III.

Mr. J. Hall-Edwards (Birmingham) projected a cinematograph film, which had been produced in Germany, showing the cycle of action of the stomach.

Memoranda:

MEDICAL, SURGICAL, OBSTETRICAL.

LEAD NITRATE FOR INGROWING TOENAIL.

I was interested in the memorandum of Dr. Liesching concerning the use of lead nitrate for ingrowing toenail, as I also have found it satisfactory. Before using it I apply a little strong carbolic acid, and have not noticed any pus under the scab.

A. Baird, M.B., F.R.C.S.Edin.,
Lieutenant-Colonel, R.A.M.C. (ret.).

Worcester.

BROMIDE ACNE TREATED BY VACCINE.

As the ordinary treatment of bromide acne seems very unsuccessful, the following notes may be of interest.

The patient, aged about 20, had been taking potassium or sodium bromide, for three or more years. Her face was covered with a rash presenting every variety of acne. The usual methods of treatment had completely failed, but it was considered inadvisable to leave off the bromide. I had written to the laboratory from which I usually obtain my vaccines, and was told that vaccine treatment of bromide acne was not likely to be of any use. In the course of an article in the British Medical Journal in June or July last year a couple of lines were quoted to the effect that Professor Strubell of Dresden had treated three or four cases with good effect. I wrote to Professor Strubell, who very kindly sent me some of the stock vaccine he used—mixed staphylococci—at the same time giving me advice on the treatment of the case.* He considers that bromide acne is almost always a staphylococcic infection, and that only in a few cases is it necessary to follow the opsonogen by a course of acne bacillus vaccine. At the same time he insists on the need of systematic local and dietetic treatment. The course of vaccine, too, is a long one, and from twelve to twenty injections, or even more, may be necessary.

In the case referred to the injections were rapidly run up to 500 million, and then maintained at that dose. Massage with ichthyol and salicylic soap was done twice daily, and a diet as far as possible free from fat carefully carried out.

Improvement was very soon noticed, no fresh spots appearing after the third or fourth dose. After a series of ten doses the skin was clear of spots, and the scars of the old ones were fading rapidly. Three weeks afterwards one or two spots appeared, and a further series of five doses was given—since which time (a year ago) no further treatment has been required, although the patient has continued to take small doses of bromides. The disappearance of the discoloration of the skin has been, from the patient's point of view, not the least valuable part of the cure.

The success of this case would make the treatment worth trying in others. I have also used "opsonogen" with very good effect in five or six cases of furunculosis, and in a case of chronic periostitis following a wound of the shin.

Mark R. Taylor.

Helston, Cornwall.

METASTATIC SARCOMA OF LUNG SIMULATING PNEUMONIA.

The clinical aspects of metastasis secondary to malignant disease of the viscera are of considerable interest and importance. When the existence of a primary tumour is obvious, or when such a tumour has been more or less

*The vaccine is made for him by the Chemische Fabrik Güstrow, and can be obtained in England from Messrs. Chas. Zimmermann and Co., of London, under the name of "Opsonogen." It has the merit of being cheap.

recently removed by operation, evidences of disease in other organs must necessarily excite more or less alarm. In actual practice, however, this relatively straightforward state of affairs by no means always exists. The primary growth may perhaps be overlooked, or the history of it may be concealed, or its nature, even though the tumour has been removed by operation, may have been doubtful. Further, the interval between the primary growth and the secondary development may be so considerable that suspicion is at rest; and in individual instances the symptoms of the later growth may be so indefinite that the true meaning may be missed. The following clinical record illustrates some of the above points:

A woman, aged 48, consulted me in September, 1912, when she complained of pain in the right side, cough and expectoration of blood-stained sputa. The physical signs were those of pleurisy and pneumonia—dullness, crepitations, bronchial breathing, etc.—with a temperature of 10° F. With rest in bed and appropriate treatment the patient gradually improved; the temperature fell to the normal level and the symptoms disappeared, so that after three weeks the patient regarded herself as completely recovered. There remained, however, at the right base physical signs which were regarded as due in part to a thickened pleura, and in part to fibrotic changes in the lung substance.

During the next two months the patient lived in the country and appeared to enjoy good health. In January, 1913, she again complained of pain in the right side, and was much troubled with cough attended by expectoration deeply stained with blood. The temperature was normal, and the tubercle bacilli was not present in the sputum. The physical signs had become much more decided. Practically the whole of the right chest was dull, and the dullness passed across the middle line in the front of the upper chest; the cardiac impulse and percussion were outside the left nipple line. In the lower part of the chest the respiratory murmur was feeble or absent, but at the higher levels the murmurs had a tubular or bronchial quality. An exploring needle was introduced, but obtained only a little blood-stained fluid. A careful investigation of the patient's history showed that in June, 1910, she was in University College Hospital under the care of Dr. Blacker, who kindly informed me that she then had "a degenerative fibroid polypus removed, and a second growth in December, 1910, the latter being of doubtful character. In April, 1911, another sloughing tumour was removed, and this had the definite structure of a sarcoma." The uterus and appendages were at once removed. In view of this history, and of the clinical facts, the diagnosis of secondary sarcoma of the lung was made. The immediate future promptly confirmed the conclusion, as the symptoms continued in spite of treatment. The patient's appetite failed, she rapidly lost flesh, and died of exhaustion on April 20th, 1913.

The feature of the case which it is desirable to emphasize is the appearance of malignant disease of the lung in the guise of an ordinary pleuropneumonia, this diagnosis seeming for a time to be confirmed by the marked improvement in the patient's general condition. Had the history of the patient been fully known at this stage it might, perhaps, have modified the prognosis, yet it could hardly have afforded ground for a confident interpretation of the pulmonary signs and symptoms as significant of malignant disease. The record lends emphasis to the caution "that with a history of malignant disease, subsequent clinical events, even though remote in time and situation, and perhaps not obviously suggestive of a secondary malignant growth, ought to be viewed with anxiety and suspicion.

Clapton, N.E. T. Muirhead Martin, M.D.

We may remind readers that the eleventh International Congress of Tuberculosis will be opened in Berlin on Wednesday, October 22nd. The first of these congresses was held in the same city in 1902; at the present congress the chief subjects for discussion will be the clinical and pathological phenomena of human tuberculosis, the surgical treatment of pulmonary tuberculosis, the influence of insurance on the campaign against the disease, and the constitution of schools for debilitated children threatened with tuberculosis.

Reports
ON
MEDICAL AND SURGICAL PRACTICE IN HOSPITALS AND ASYLUMS.

MONTROSE ROYAL ASYLUM.

A CASE OF RETENTION OF FAECES WITH UNUSUAL SYMPTOMS.

(Reported by John G. Havelock, M.D.Edin., Physician-Superintendent.)

A. I. S., female, aged 38, single, domestic servant, was admitted on the night of August 29th, 1913, a pauper patient from a remote parish in the north of Scotland.

The history obtained was that she had been detained in an English county asylum from October, 1912, to July 26th, 1913, when she was removed to Scotland and placed in a poorhouse. She was found to be too troublesome for management there, as during the night she shrieked and shouted for no apparent reason, and refused to pass urine so that it was necessary to have it drawn off by catheter. The medical certificates stated that "she was alternately quiet and sullen, and restless and excited; talked incoherently and kept staring with a wild, vacant look in her eyes. At other times she sat silent and brooding, refusing to speak. She suffered from retention of urine, but objected to urine being drawn off although none had been passed for twenty-four hours."

Her mental condition on admission was that described in the medical certificates; her bodily condition was about average, but it was noted that the abdomen was distended. A catheter was passed, but only some 8 oz. of urine were drawn off. Her weight was recorded as 9 st. 9 lb.

On the morning of August 30th a dose of Epsom salts was administered, but this was vomited, and later in the day a dose of castor oil, which she retained. As this was not followed by an action of the bowels an enema was administered, and it was then manifest that the abdominal distension was due to retention of faeces. Copious enemata and mechanical operations succeeded in removing an immense accumulation of scybala, which were emptied into the ward w.c., with the somewhat ludicrous sequel that the drain became choked and was only relieved after much strenuous work by the institution plumber.

As the patient had been carefully weighed just before the operation, when the scale showed 9 st. 9 lb., it was evident that a similarly careful record of her weight after the removal of the faecal matter would give a correct estimate of the amount which had been removed. It was surprising to find that she then scaled only 8 st. 10 lb., so that the evacuated mass was 13 lb. in weight. Surely this must be a record stool; but I remember a somewhat similar case which occurred here when a patient, soon after admission, passed unaided a solid stool exactly the size of an ordinary whisky bottle, and of the same shape, minus the neck.

The case shows that faecal retention of a serious nature may exist without showing the usual well-known symptoms. In this instance two very able general practitioners seem to have been misled by the patient refusing to empty the bladder; and it was only by the observance of the routine precaution of ensuring that the action of the bowels is regular that we were able promptly to diagnose the trouble.

The case also may be regarded as an example of the extreme difficulty that exists in the medical treatment of the insane; the patient can give no assistance, and the history of the case is usually not only meagre but often absolutely misleading.

Dr. J. M. Mullick and Dr. B. N. Ghosh, writing on behalf of the Editorial Committee of the *Indian Medical Record*, Calcutta, informs us that a special tuberculosis number of that periodical will be issued in December, and that they are prepared to deal with notes on prevention and treatment based on personal experience if received by the middle of November. The editors have been moved to take this course by the prevalence of tuberculosis in India, the serious mortality it produces, and the rapid progress which the disease is said to be making.

Reviews.

SOME CLINICAL TEXTBOOKS OF TUBERCULOSIS AND TUBERCULIN.

All modern students of tuberculosis must by this time be familiar with the associated names of Bandelier and Roepke. Together and separately these two physicians have contributed much to the literature of the subject, and their exceptional opportunities as directors of large sanatoriums have given them the right to speak with authority on the results of investigations now extending over a considerable period. Their most ambitious work, descriptive of the clinical aspects of tuberculosis as affecting all parts and organs of the body, has proved so useful to the profession in Germany that a second edition has been issued within a comparatively short time, and a translation of it by Dr. Bertram Hunt has recently been prepared for the profession in England, and presented in the form of a handsome volume entitled, *A Clinical System of Tuberculosis*.[1] Covering the whole range of tuberculous disease, the work is conveniently arranged for reference. It is properly recognized that tuberculosis, wherever it may occur, is a pathological entity, and although it can be more easily studied in one part than another it is, in its main features, amenable to the same scheme of treatment. In preparing the second edition. the authors have taken full advantage of the criticisms and suggestions that were evoked by the first, and have noted all recent advances both in pathological and therapeutical respects. As a practical guide to the better understanding of the present state of knowledge on the subject, the book in its English dress should prove of real value to the very large number of persons who are now concerned in the recognition and rational treatment of this scourge of the human race. The average reader may be disappointed to find that there is so much difference of opinion upon certain important points between equally skilled observers. Even as to the paths of entry open to the bacillus there is no common agreement. A rough division into aërogenic and enterogenic may be made for controversial purposes, but the finer distinctions are not so easy to explain. The authors have made careful selection of the leading authorities for quotation, leaving the reader to form his own judgement on many moot points. Pulmonary tuberculosis naturally claims the first place for consideration, and no stone with respect to it is left unturned. Like many other physicians who are in daily contact with a special disease, Drs. Bandelier and Roepke are by no means prepared to exalt the microscope and the cultivation plate above the older instruments and methods of diagnosis. For the practical purpose of early diagnosis, for instance, they lay great stress upon inspection and palpation, and evidently regard the phenomena of agglutination, fixation of complement, and Bauer's anaphylaxis with considerable suspicion, as too inconstant in their occurrence to inspire trust. As experienced sanatorium managers they have much to say on the subject of rational treatment, and their detailed advice is worthy of close study. The general lines of physical treatment are well known, but very many modifications of them can only be learnt by experience, and almost every case calls for careful consideration of some special feature. In agreement with many others of like experience, they consider that at the present time the association of careful tuberculin treatment with enforced hygiene is the most successful line to adopt. As in their previous work on tuberculin in diagnosis and treatment, they utter grave warnings with regard to the necessity for careful consideration before employing inoculations, and maintain that every one proposing to inject any preparation of tuberculin ought to make himself fully acquainted with the exact qualities of the preparation that he is about to use, and not to trust to the "instructions on the bottle." Various surgical methods have of late years been suggested and practised for the relief of advanced cases—such, for instance, as the production of artificial pneumothorax with injection of nitrogen into the pleura, resection of ribs, etc.; but of all these they speak with great reserve, and only recommend their use when all resources of medicine are exhausted. The provision of State hospitals for consumption in Germany, at first disfavoured, would appear now to be recognized as useful to the community in many ways, but chiefly as a ready means for isolation of infective cases. It has been found that segregation is not objected to so long as some form of treatment is adopted. The *spes phthisica* has often proved stronger than nostalgia. Only passing mention is made of national insurance in Germany, but enough is said to show that it is applied strictly to cases with some prospect of future usefulness and not indiscriminately. Space does not permit of mention of the many practical points arising in the course of the authors' account of tuberculosis of the various systems and organs, and it must suffice to say that the same strain of practical common sense is traceable throughout, giving the work a value, not only as a complete account of the whole subject, but as a trustworthy aid to the average practitioner in his daily round of work. Much credit is due to the translator for the pains that he has taken to render the authors' meaning in clear and readable English.

The earlier work of Bandelier and Roepke, dealing with *Tuberculin in Diagnosis and Treatment*,[2] has already passed into a seventh edition, and an excellent English translation of it has been made by Dr. Wilfred B. Christopherson of Arosa. An earlier English edition, translated from the second German issue by Dr. E. G. Morland, was published in 1909, but so much work has been done by way of research, and so much clinical experience has been gained, that a fresh account of the whole subject is fully justified at the present time. An introductory note was written for the third edition by the late Professor Koch in which he rightly prophesied that the sterling character of the work would find recognition and call for many subsequent editions. Produced in the same style, though not quite uniform, with the larger work referred to in the foregoing review, the book possesses all the qualities that we have noted in its companion. It is divided into three main sections, dealing first with the theoretical aspects of tuberculin treatment, then with its value in diagnosis, and finally with its practical application in the treatment of the disease in all its varying conditions and localizations. Each branch of the subject is dealt with at length, and every observation or train of thought that has been put forward by investigators of repute finds mention and criticism. It has, however, to be admitted that no commonly acceptable theory has as yet been reached, and that each must stand or fall on its own merits. A few definite points have been established. It is agreed that the bodies of all tuberculous individuals contain antibodies, and that the phenomenon of hypersensibility varies greatly. The latter phenomenon has been subject to much study, but as yet no laws can be formulated with respect to it. The idea that immunity artificially acquired is not an advantage to a tuberculous patient is not shared by the authors, and throughout the work it is made very clear that increase of experience has inspired increase of faith in the value of tuberculin judiciously administered. With respect to its diagnostic value there is much to be said, but the conclusion arrived at brings the reader back to the point from which he started, and he is advised to pin his faith, as heretofore, to the subcutaneous method as likely to produce the most trustworthy evidence of the presence of tubercle, and to trust his ordinary diagnostic powers to determine the extent or position of the focal lesion. In the case of children, the less painful cutaneous method may be preferable, but many reservations attach to its indications and call for very cautious interpretation. The major part of the book is devoted to an account of the results hitherto recorded, mostly by German observers. Tuberculosis of every part is dealt with, and the arrangement of the subject renders the whole very easy of reference. The outcome of such a collation

[1] *A Clinical System of Tuberculosis; describing all Forms of the Disease.* By Dr. B. Bandelier, Medical Director to the Sanatorium Schwarzwaldheim at Schomberg, and Dr. O. Roepke, Medical Director to the Sanatorium for Railway Workers at Stadtwald, Melsungen, Cassel. Translated from the second German edition by G. B. Hunt, M.D. B.S., late Physician to the Scarborough Hospital. London: John Bale, Sons, and Danielsson, Limited. 1913. (Roy. 8vo, pp. 537; 8 coloured plates. 21s. net.)

[2] *Tuberculin in Diagnosis and Treatment.* A Textbook for Practitioners and Students. By Drs. Bandelier and Roepke. Second English edition translated from the seventh German edition by Wilfred B. Christopherson of Arosa. London: John Bale, Sons, and Danielsson, Limited. 1913. (Roy. 8vo, pp. 321; plates, illustrations, and charts. 15s. net.)

of contemporary study and research is somewhat disappointing, and the conviction is forced upon the reader that no great advance has been made of late years towards a common agreement upon any of the points at issue. The authors, themselves men of exceptional experience, can only maintain that tuberculin does some good, even to advanced cases, and that no positive rules can be laid down to guide its employment. Personal experience and opinion must still determine the treatment in each case, and the need for caution is as great as ever. They urge very strongly the advisability of careful study on the part of practitioners who have to carry out special treatment, and recommend a few months' sojourn in a sanatorium as the best means of acquiring the needful experience. But as a guide to the present state of medical thought on this very difficult subject the book cannot be praised too highly. The translator has put the whole work before the English-speaking profession in a form which should earn the gratitude of all who wish to keep pace with the progress of modern research.

THE DIAGNOSIS OF SMALL-POX.

ON page 9 of his book, *How to Diagnose Small-pox*,[3] Dr. WANKLYN quotes a statement by Dr. Ricketts that "small-pox is, perhaps, of all diseases that in which a certain diagnosis can be arrived at in almost every case." With this saying Dr. Wanklyn writes that he is in agreement. How is it, then, that mistakes are made? The answer to this question is supplied, first, by the fact that few medical practitioners and students in these days have the opportunity of studying small-pox at the bedside, because for some time past cases of the disease have been scanty in this country, and such as have occurred have been hurried off to special isolation hospitals, in which they have been lost so far as clinical teaching has been concerned; and, secondly, by what, we fear, is also the fact that the method of diagnosis usually employed is faulty. Dr. Wanklyn points out that the characteristic features of small-pox lie mainly on the surface throughout the course of the attack, and are not hidden away internally and obscurely. This makes for certainty in diagnosis in spite of admitted difficulties. These features and the proper method of studying them are very clearly described by the author. We strongly advise that a perusal of the book should be undertaken at once, and not deferred to the next outbreak of small-pox which the unvaccinated condition of London and other large cities in this country is certain to bring about, very probably at an early day. The book is small and inexpensive, its language is simple, its contents can be mastered in an hour or two. The misdiagnosis of a case of small-pox has so frequently led to deplorable results that it is the duty of every medical man, in the interests of the public health as well as of his own reputation, to be as conversant with the disease as is possible under the circumstances. He will find this book most valuable for the purpose.

THE INDIAN JOURNAL OF MEDICAL RESEARCH.

THE *Indian Journal of Medical Research*, the first number of which bears date July, 1913, makes a bold bid to take its place among the leading periodicals devoted to the publication of the results of medical research. It is published for the Indian Research Fund Association by Messrs. Thacker, Spink, and Co., Calcutta, and will appear quarterly. The subscription price in India will be 6 rupees, and in other countries 10s., including postage in either case. It will replace the periodical, *Paludism*, the first number of which was issued in July, 1910, and also the great majority of the monographs published in the *Scientific Memoirs by Officers of the Medical and Sanitary Departments of the Government of India*, a series which has been published for many years at irregular intervals.

The Indian Research Fund Association exists for "the prosecution and assistance of research, the propagation of knowledge, and experimental measures generally in connexion with the causation, mode of spread, and prevention of communicable diseases." The new journal will deal with every branch of research directly or indirectly connected with medical and sanitary science, including hygiene, sanitary engineering, sanitary laws, statistics, bacteriology, parasitology, and entomology. It will not publish reviews or abstracts of papers published elsewhere, but it will include from time to time reviews of existing information regarding particular diseases, such as the article in the first number by Captain J. Cunningham, I.M.S., summarizing present knowledge of dysentery, and giving a full bibliography of the subject. Summaries of official reports of the Indian Government containing matter of scientific medical interest will also be published. The main intention of the founders of the new journal, which will be edited by the Director-General, I.M.S., and the Sanitary Commissioner with the Government of India, is to present in a readily accessible form accounts of the work done in India with the object, not only of letting the outside world know how much is being done, but also to co-ordinate such work and excite healthy emulation.

Of the sixteen papers in the first number, five are concerned with medical entomology. The first, by Captains W. S. Patton and F. W. Cragg, of the King Institute, Madras, deals with certain haematophagous species of the genus *Musca*, and contains a description of two new species. The second paper by the same authors describes a new species of *Philaematomyia* and contains some general remarks on the genus. Mr. F. M. Howlett, in a paper on the natural host of *Phlebotomus minutus*, the commonest sand fly in Bihar, and a pest to man in summer, shows reason for believing that its natural host is the common Geckonine wall lizard. Major E. D. W. Greig, I.M.S., who is on special duty for the Cholera Inquiry, contributes a series of papers on cholera. The first is on an investigation into the occurrence of the cholera vibrio in the biliary passages, the second on an epidemic of cholera caused by a "carrier," the third on an investigation of cholera convalescents and contacts in India, and the fourth on the occurrence of the comma bacillus in the urine of cases of cholera. Major A. C. MacGilchrist has a paper on the haemolytic action of quinine and its salts, containing suggestions regarding the etiology and treatment of blackwater fever. Lieutenant-Colonel C. Donovan, I.M.S., contributes a paper on kala-azar in which he discusses its geographical distribution, showing that its endemic areas are low-lying damp districts near rivers, places very suitable for malaria and where in fact the two diseases coexist. He discusses also the probable mode of infection without arriving at any very definite conclusions, but he exculpates the house-fly of India, and suggests that attention should be directed to the possibility of the conveyance of *Leishmania* by food contaminated by excreta or other substances containing the encysted forms of the flagellate. His paper is followed by a preliminary report by Captain W. S. Patton on the question whether kala-azar in Madras is of animal origin. The general conclusion of this research so far as it has gone, is that the dog and probably the cat play no part in the transmission of the disease in Madras.

This first number of the *Indian Journal of Medical Research* is well illustrated by reproductions of photographs in the text and by special plates of flies from drawings signed Edith M. Patton which attain a very high standard of excellence; it may be worth while to add that in the copy we received they were not very securely fastened. Subsequent issues of the new periodical will be looked forward to with high expectation by all interested in the study of the diseases of hot countries.

VITALISM AND REPRODUCTION.

PROFESSOR MARCUS HARTOG'S volume entitled *Problems of Life and Reproduction*,[4] one of the Progressive Science Series, consists rather of a series of essays than of a consecutive exposition. Several of the chapters have been previously published as contributions to scientific journals or reviews. The last chapter is devoted to the teaching of "Nature Study," and was delivered as an address to

[3] *How to Diagnose Small-pox*. By W. McC. Wanklyn, B.A.Cantab., M.R.C.S., L.R.C.P., D.P.H., Assistant Medical Officer of the London County Council. London: Smith, Elder, and Co. 1913. (Demy 8vo, pp. 111; 6 plates, 4 diagrams, 1 chart. 3s. 6d. net.)

[4] *Problems of Life and Reproduction*. By Marcus Hartog, M.A., D.Sc., F.L.S., F.R.H.S., Professor of Zoology in University College, Cork. London: John Murray. 1913. (Demy 8vo, pp. 379, 41 figures, 3 tables. 7s. 6d. net.)

teachers in Queen's College, Cork; this chapter has but little relation to the subject-matter of the remainder of the book.

The work is primarily addressed to the lay reader, but there is much in it which, though not new, will prove of interest to the biological expert. The earlier chapters deal with the problems of the origin of the sexual process and of the bisexual cells. The evolution of the latter is traced from the green flagellates through an interesting series of the *Fucaceae* to the higher types with definite ova and spermatozoa. These facts lead on to a consideration of the essential processes of conjugation and fertilization which, in the opinion of the author, "lies in the creation of a new cell, whose nucleus and protoplasm have not been previously associated in a common cell life." In this connexion there is an interesting discussion on the interrelationships between the nucleus and the protoplasm of cells in general.

Passing from sexual reproduction to the processes of reproduction of organisms by grafting, budding, and the regeneration of lost parts, Professor Hartog next deals with the bearing which these facts have upon the theories of inheritance. The views of Weismann and of Herbert Spencer, Darwin's pangenesis and the facts of parthenogenesis are briefly set forth. Weismann's germ-plasm theory and the non-inheritance of acquired characters are somewhat severely criticized. The number of biologists who believe in the inheritance of acquired characters if few are steadily increasing, and Professor Hartog is to be numbered among them.

An interesting and readable account is given of the physical factors concerned in the process of mitosis and of the new dual heteropolar force—mitokinetism. The view is expressed that much of the persistent advocacy of the view that mitokinetism is identical with electrostatic force "is due to a latent fear that a 'new force' within the organism, not known in the inorganic world, might strengthen the side of the vitalists."

Professor Hartog confesses himself to be a vitalist, and the brief discussion of the subject of vitalism will be read with interest at the present time owing to the general attention drawn to it at the recent meetings of the British Association.

The latter part of the book deals with such subjects as instinct and memory, and more particularly with the life and writings of Samuel Butler, for the new edition of whose work on *Unconscious Memory* Professor Hartog wrote an introduction.

The Problems of Life and Reproduction is a volume which will well repay the time spent in reading it. The author's interest in this branch of biology is well known, and no more competent writer than the Professor of Zoology in University College, Cork, could have been found to put the subject before the non-scientific reader. To the expert biologist there is much in it which will be considered heterodox, but this is an advantage. Too rigid orthodoxy in science does not tend to progress. There is one point, however—a comparatively small one—which does call for adverse criticism; it is the "legends," and more particularly the lettering, of the illustrations, which are in much too small type. Fig. 7, for instance, almost requires the use of a magnifying glass. This fault might easily have been avoided. Otherwise, the book may be confidently commended to all those interested in the problems with which it deals.

NATIONAL INSURANCE IN GERMANY AND ENGLAND.

At the Conference of the American Hospital Association held in Boston, Mass., in August last, a paper contributed by Dr. Mackintosh of the Western Infirmary, Glasgow, was read which he has in a pamphlet entitled, *National Insurance in Germany and England*.[5] The paper gives some interesting details, with statistics, of the arrangements in Germany for medical benefit and the struggles between the medical profession and the societies as to free choice of doctor and the amount of medical remuneration. In commenting on the fact that with free choice of doctor in Germany there has been a great increase in the sickness claims, Dr. Mackintosh utters a warning, which he thinks is specially applicable to areas where there is the system of payment by attendance, that unless the greatest care is shown in the granting of certificates in England, the experience of Germany leads him to believe that the demand for State appointed referees and for the administration of medical benefit by the societies will be likely to increase. In this connexion he quotes figures about malingering in Leipzig where the societies pay a capitation fee of 5s. for medical treatment of insured persons without dependants, or 7s. 3d. when attendance on dependants is included. The money is handed over to the two medical associations and pooled, the doctors being paid by attendance, with a *pro rata* deduction if the pool is insufficient to pay in full.

The Leipzig societies have their own confidential medical advisers to examine patients suspected of malingering, and in 1910 the number of persons referred for examination to ascertain whether they were really unable to work was 8,487. Of these requests for examination 3,382 came from the doctors treating the patients. Over 2,600 of the total number either notified their recovery before the examination or failed to appear, and out of 5,827 persons actually examined, 47 per cent. were declared able to work at once, 12 per cent. able to work at the end of a week, 10 per cent. were to be re-examined at the end of a week or two, and 31 per cent. were declared unable to work.

After dealing with the German system the main provisions of the National Insurance Act are described and the panel system explained; attention is drawn to the facts that in England only treatment within the competence of a general practitioner is given, while in Germany payments to specialists are made, and that treatment of dependants is already given in Germany.

Speaking of the hospital treatment of the insured, Dr. Mackintosh points out that in Germany the public authorities own general hospitals in which patients of all classes are received and payments are made for all, the charges being fixed by each authority. The societies take advantage of this, as they may give their members treatment in hospital in place of medical and money benefits, the latter being reduced while the member is in hospital. The prices charged to the societies work out at below cost. For example, at Kiel, where the cost for treatment and maintenance, exclusive of capital cost, is 4s. 1d. a day, the Kiel District Sickness Society only paid an average of 2s. 7d. a day for its members in hospital. In England, Dr. Mackintosh says, the Insurance Act was apparently drafted on the understanding that the hospitals would continue to give the best treatment without charge to the necessitous, and there is no provision making it incumbent on the societies or insured persons to pay for hospital treatment. In two cases only, he said, did the Act take cognizance of hospitals—first, where it allows societies to give donations; and, secondly, where it provides that if an insured person without dependants is an inmate of a hospital, the society may enter into an agreement with the hospital to pay the whole or part of the person's benefit to the hospital. Dr. Mackintosh feared that, if that were done, the Local Government Board might demand that the hospital should be open to inspection, that special records of the cases would have to be kept, and that any changes in the management or alterations in the medical staffs would have to be notified to the Local Government Board. Dr. Mackintosh expressed the opinion that this would strike at the root of the voluntary principle, and that managers would hesitate to hand over their institutions to be so governed. On July 27th Mr. Masterman said in the House of Commons that, if the insured person were willing, some of the benefit could go to the hospital, but he would be very chary in making over to the hospital the whole of the benefit; he thought there ought not to be any pressure on the patient to sign away his sick pay to a hospital, and he moved an amendment which would ensure that the benefit should be paid to the patient upon leaving an institution in a lump sum or in instalments, or applied as the society or committee thought fit. Dr. Mackintosh concludes: "The question arises, Will the voluntary principle stand this strain? For my part, I see no reason in the meantime to suppose that voluntary hospitals in large cities will not be maintained in the future as they have been in the past, but only time and

[5] *National Insurance in Germany and England.* By Donald J. Mackintosh, M.V.O., M.B., LL.D., F.R.S.E. The American Hospital Association. (Printed for private circulation.)

experience will confirm this belief." Dr. Mackintosh acknowledges his indebtedness to the work on *Medical Benefit in Germany and Denmark*, by Mr. I. G. Gibbon, which was reviewed in the JOURNAL of October 26th, 1912, p. 1151; and also to Mr. W. R. Fuller, of the Nurses' Insurance Society, for his assistance.

GRAY'S ANATOMY.

Gray's Anatomy[6] is so well known to medical students and practitioners that the appearance of the eighteenth edition calls for little comment beyond pointing out the main alterations which Professor HOWDEN, the acting editor, has introduced. The outstanding modification in this edition is the use of the Basle nomenclature. Except in one or two instances this nomenclature has been adopted in its entirety; in most cases English translations of the Latin terms are employed, but in those cases in which the Latin terms have become fixed by routine usage the editor has retained them. Thus the new terminology has been adopted by the four leading textbooks of systematic anatomy used in this country—namely, Quain, Cunningham, Morris, and now Gray. Obviously it has come to stay, and we must be prepared to learn it whether we will or no, but the most enthusiastic supporter of the findings of the Basle Commission will admit that before long alterations and modifications will become necessary.

An improvement in this edition will be found in the section devoted to surface anatomy. In previous editions certain paragraphs relating to this division of anatomy were appended to the descriptions of the various structures. These have now been collected and recast into a special chapter, an arrangement well adapted for easy reference.

When a book has reached the eighteenth edition criticism and suggestion seem unnecessary, for it is obvious that the writers and readers are so intimately in touch with one another that the requirements of the latter are continuously being met. When *Gray's Anatomy* first appeared its subject matter occupied 750 pages, and the illustrations numbered 363. Now the subject matter extends to 1,243 pages, with 1,120 illustrations, of which 431 are coloured. There can be no doubt that the volume will continue to hold the high position it has gained as one of the very best textbooks of human anatomy, and we offer our congratulations to Professor Howden on his skill in so admirably adapting the book to modern needs.

NOTES ON BOOKS.

PROFESSOR LEDUC of Nantes has written a second volume in his series of studies in what he calls biophysics. His first volume was translated into English by Dr. Deane Butcher, and published under the title, *The Mechanism of Life*. The new volume, with the title, *La biologie synthétique*,[7] continues the subject, and relates several series of experiments in which by the chemical and osmotic interaction of a concentrated salt with a solution of another, various forms are produced which mimic lichens, seaweed, and other vegetable structures. The most remarkable of his results are, however, no doubt those which show appearances resembling karyokinetic figures. Professor Leduc gave a demonstration of these appearances at the meeting of the British Association in Dundee last year, and photographs are reproduced in both his books. His investigations are of great interest as throwing light on the physico-chemical processes which underlie the phenomena of cell division and the forms of living things. The volume is very fully illustrated, and the author has stated his facts and arguments with the clearness and precision characteristic of the best French scientific writings.

The thirty-third volume of the *Transactions of the Ophthalmological Society of the United Kingdom*[8] relates to the proceedings of this body during the session 1912-13 and contains some forty papers, many of which are illustrated by plates or diagrams. Three of them relate to retinal changes associated with general disease, four to diseases of the eye and conjunctiva, three to those of the orbit, four to those of the cornea, and six to those of the uveal tract. The rest deal with diseases of the vitreous, with congenital abnormalities of an ophthalmic order, with questions of refraction, glaucoma, and nerve defects. Among the more noteworthy papers are Mr. Bishop Harman's analysis of 300 cases of high myopia among school children, and those on cases of optic atrophy of familial type by Drs. James Taylor and Gordon Holmes.

The manual dealing with procto-sigmoidoscopy and its use in the diagnosis and therapy of rectal and sigmoid diseases, written by Professor STRAUSS,[9] to whom we are indebted for the instrument that has made the procedure of real value, is one that will well repay perusal by all those—and they surely include almost every surgeon—who have to deal with the obscure symptomatology of disease in the lower intestinal tract. Anything which makes it possible to anticipate the tragedies associated with undetected carcinoma of the bowel is sure of universal welcome. The sigmoidoscope has its detractors, and disasters have attended its use in inexperienced hands and in unsuitable cases. This does not in the least detract from its value in competent hands and in proper cases. A much more serious point is that when the correct technique has been learnt, as it may be from the very clear and definite directions given in this book, there remains the difficult task of interpretation of the findings. It is a difficulty inseparable from all instruments disclosing but a small field, at what, for ocular purposes, is a considerable distance, in tracts not ordinarily visible, of conditions that look quite different from what is seen on the *post-mortem* table or in museum specimens. Here Dr. Strauss expounds the results of his great experience, and, moreover, gives a series of good coloured plates of sigmoidoscopic views of both normal and diseased conditions. Practically the only contraindications to the use of his instrument recognized by the author are acuto inflammatory conditions immediately around the rectum or in the vicinity of the sigmoid as it lies when the tube is in place. Strauss prefers the knee-chest position for examination, and only very rarely finds it necessary or advisable to employ narcosis; sometimes a local anaesthetic is needed, but with good lubrication the instrument can be used almost painlessly in most cases. It is to be remembered that even expert inspection may err in estimating the innocence or malignancy of ulcers and that excision of small portions for histological examination is not only possible, but imperative.

Some twenty-five papers read at the annual meeting in April last year are contained in the twenty-fourth volume of the *Transactions of the American Pediatric Society*.[10] Some half-dozen deal with the feeding of infants on strictly scientific lines, among them being one in which Dr. David Cowie describes very fully a graphic chart method of studying the subject. Dr. Freeman of New York supplies an account of 10 cases of pneumonia in infants treated by a serum without any conspicuous benefit, and Dr. Griffith an analysis of 75 cases of typhoid fever in infants. Also worth noting is a study of the Wassermann reaction in infants and children which led the author, Dr. Churchill, to conclude that congenital syphilis was common in Chicago, and a paper on the employment of salvarsan in children and infants by Dr. L. E. La Fétra, who is also the editor of the volume.

Under the title of the "S.P. Pocket Guides,"[11] the Scientific Press, Limited, is now issuing a series of booklets of vest-pocket size intended for use as reference books by those already trained in the subjects to which the booklets relate. Those already published or in the press deal with such questions as fever nursing, Swedish exercises, bandaging, the reading and writing of prescriptions, drugs and their uses, the securing of asepsis, operation after-treatment, and the duties of nurses before and during an operation. Some of the authors are medical men.

[6] *Anatomy, Descriptive and Applied.* By H. Gray, F.R.S. Eighteenth edition. Edited by R. Howden, M.A., D.Sc., M.B., C.M. Notes on Applied Anatomy revised by Dr. A. J. Jex-Blake, M.A., M.B., B.Ch., F.R.C.P., and W. F. Fedden, M.S., F.R.C.S. London: Longmans, Green, and Co. 1913. (Roy. 8vo, pp. 1325; figures 1120. 32s. net.)

[7] *La biologie synthétique.* Par Stéphane Leduc. Paris: A. Poinat. 1912. (Cr. 8vo, pp. 218.)

[8] *Transactions of the Ophthalmological Society of the United Kingdom.* Vol. xxxiii. London: J. and A. Churchill. 1913. (Demy 8vo, pp. 240. 12s. 6d. net.)

[9] *Die Procto-Sigmoscopie.* Von Professor Dr. H. Strauss. Leipzig: Georg Thieme. 1910. (8. roy. 8vo, pp. 149; 92 illustrations, some coloured.)

[10] *Transactions of the American Pediatric Society*, vol. xxiv. Edited by Dr. Linnaeus Edford La Fétra. Chicago: American Medical Association Press. (Medium 8vo, pp. 318.)

[11] The S.P. Pocket Guide Series. London: The Scientific Press, Ltd. Each volume is bound in limp cloth, 1s. 1d. post free.

SANITARY AND DOMESTIC APPLIANCES.

A New Gas Fire.

THE domestic gas fire is still in an experimental stage, and not yet as efficient and economical as doubtless it is destined to be. A considerable step forward, however, has been taken by Dr. Charles Gayford, of London, who has recently patented an improvement in gas fires which seems likely further to simplify and popularize this method of warming a living-room. The primary object of the invention is to adapt the ordinary fire-grate to the use of a combination of gas and air as fuel. On inspecting the device it is seen that the fire-brick back and sides of the grate are rearranged according to a special plan, one result of which is that the back presents a plane surface slanting forwards, at an angle of about 60 degrees to the floor level. Along the floor by the side of the fireplace is laid the copper or iron burner, with a mechanism at one end for admitting the air supply into the joint service pipe, and for regulating such supply by a revolving shutter. At the other end the burner widens and flattens out into an 8-in. slit opening, which emits a steady sheet of flame. This head of the burner is introduced into a suitable aperture in the grid bottom of the grate, and the flame is made to strike upon the slanting surface of the firebrick back, which soon becomes red-hot, and stores the heat in its substance. With the continual impingement of the flame upon this back, the heavy hydrocarbon vapours, which are the constituents of coal-gas, are disintegrated, and complete combustion is said to be practically instantaneous; the residual end products of such combustion, namely, carbon dioxide and steam, escape by way of the chimney. The radiation of heat from this firebrick back, through the usual pile of fireclay, is naturally most intense in the lowermost stratum of air, and this stratum, rising as it is warmed, is replaced from the air inlets of the room by successive cool layers, which rise in their turn. Dr. Gayford showed us some figures which indicated that a twelve hours' constant and efficient fire could be kept up for a consumption of about 177 c.ft. of gas, which in respect to gas consumption, and consequently to cost of maintenance, represents a saving of 50 per cent. on that of the ordinary gas fire. The points upon which the inventor of this gas fire insists are that the open chimney is still available as a mean of unobstructed ventilation, that the gradual upward warming of the air does away with the unpleasantness of down draughts, that no fumes given off by partially consumed coal-gas are retained in the room, and that the invention has its obvious bearing on the question of smoke abatement.

NATIONAL GAS CONGRESS AND EXHIBITION.

A GASOMETER is an ugly object which gives the approach to many towns a sordid aspect, but that the subject of gas can be made really attractive, and that an intellectual and artistic pleasure attaches to many phases of the gas industry, is demonstrated at the present National Gas Exhibition at Shepherd's Bush. We should like to think that this display inaugurated a new era in exhibitions, for it is an exhibition with only one exhibitor—namely, the gas industry of this country—and individual competition has been successfully damped down. No salesman's importunities worry the visitor, nor is he distracted by the rival claims of competing exhibits. Moreover, although the distance is said to be half a mile from one end of the corridor and halls to the other, he is conscious of no fatigue, for the reason that some one has realized that the exhibition-goer likes to sit down occasionally. But, over and above all these advantages, the exhibition is pleasing to the eye. The illuminator and fitter has been assisted by the architect, and the upholsterer, and the paperhanger, and the expert in draping, but the greater part of the pleasure after all is due to the cheeriness and warmth of the gas itself. There is no doubt that for the pleasing appearance of the exhibition the British Commercial Gas Association is largely responsible. This body, which is supported by companies and corporations supplying gas, organizes demonstrations on a large scale, and otherwise advertises the industry. Those who remember the old days when, as Sir Corbett Woodall reminded the company at the opening ceremony, anything like canvassing on the part of a gas company would have been considered unnecessary if not distinctly vulgar, can only marvel at the array of brightly illustrated pamphlets which the association has scattered about the exhibition, and at the seductive advertising which pleads the merits of gas at every turn.

A detailed description of the exhibits is out of the question. The visitor will pass through a large hall filled with every kind of gas engine and appliance for industrial purposes; he will see also an arcade of shops, an attempt at a church interior, a class-room scientifically lighted, various stalls to demonstrate the scientific side of gas as an illuminant, such as the effect of differently tinted wall-papers on a lighted interior, a large town house, furnished and illuminated from billiard room to garage, and, by the side of this, a small dwelling fitted with a prepayment meter. Among the exhibits is a section of a hospital ward, showing the sanitary appliances and the hot-water apparatus, and a bath-room and nurses' sitting-room adjoining, the latter possessing a gas-fire and a small cooker. The ward itself is designed to show that the radiant gas-fire helps in the ventilation, assists in maintaining cheerfulness, and offers itself to convenient adjustment for keeping an equilibrium of temperature.

The exhibition was opened by the Lord Mayor of London, supported by a large number of provincial mayors, and several of the speakers on that occasion, notably the Lord Provost of Edinburgh, laid stress upon the bearing of the gas question on public health, notably in the direction of smoke abatement. In the list of patrons of the exhibition we have counted the names of no fewer than eighty medical men, and among those present at the luncheon on the opening day were Sir Alfred Keogh, who replied to the toast of "The Visitors," Sir John Collie, Dr. Leonard Hill, Professor Henry Kenwood, and Dr. Meredith Young.

Conference on Food and Cookery.

The first of the conferences to be held during the course of the exhibition took place on October 4th, the subject being cookery, chiefly in its relation to health. In delivering the opening address, Dr. Joseph Priestley, M.O.H., Lambeth, treated what the author of the *Anatomy of Melancholy* calls "a noble science" with becoming respect. Despite the jest of Garrick—that Heaven sent the meat and the devil the cooks—civilized man could not get on without their assistance. The aim of cooking was to aid digestion, and just as in digestion there was a mechanical process, represented by mastication and peristalsis, and a chemical process, represented by the action of digestive juices and ferments, so cooking also included a mechanical process in which the fibres of meat and other foodstuffs were loosened and softened, and a chemical one, in which certain changes, such as the partial coagulation of albumin and emulsification of fat, were started, to be carried further and completed by the physiological functions of the body. Cooking increased the digestibility of vegetables, principally by softening their celluiose network, but lessened the digestibility of meat by partially coagulating the albumin and by causing the meat to shrink in bulk, owing to loss of water. The lessening of digestibility, however, did not vitiate the value of cookery, for it prevented the too-rapid absorption of the resulting chyle into the blood stream, and—like vinegar and other condiments, as well as tea, coffee, and alcohol—it damped down the human furnace. Formerly there was an idea, abandoned now for some years by the expert, and at last thrown over by the layman, that gas-cooking tended to poison the meat, owing to the escape of fumes. The gaseous products were not absorbed by the meat, for the reason that meat during cooking was emissive, and this pressure from within effectually prevented imbibition. The only drawback to the gas-cooker was the expense of the fuel; but otherwise the result might satisfy the dock labourer's wife and the most fastidious chef. The appearance of the food and its taste were the same as when cooked by the coal fire, as were also the chemical changes it had undergone, though these last were brought about by somewhat different methods. The scientific method of cooking meat by roasting or boiling was to submit it first to a high temperature in order to obtain the preliminary coagulation of the outer surface, thus retaining the juices within this jacket, and to follow this up by the moderate but prolonged and uniform application of heat. For this purpose the gas stove was perfect, and its value for the early breakfast or the late supper, or for the emergency of sickness in the night, was obvious.

The other papers of the afternoon dealt rather with gas cookery from the standpoint of domestic economy, but mention should be made of Miss Mildred Atkinson's excellent communication, in which the kitchen coal fire was arraigned as one of the causes for a great deal of the existing indigestion and malnutrition, and incidentally for the vogue of quack medicines. The ignorance of dietetics and food values on the part of the housewife among the poorer classes was very largely to blame, but the absence of labour-saving machinery in the home, in the first rank of which was the gas-cooker, virtually condemned her to drudgery, inefficiency, and discomfort.

THE OPENING OF THE WINTER SESSION.

(Concluded from page 878.)

EXAMINATIONS, EXAMINERS, AND EXAMINEES.

ADDRESS BY SIR WILLIAM OSLER.

On the first day of the winter term Sir WILLIAM OSLER, after distributing the prizes at St. George's Hospital Medical School, delivered an address on examinations, examiners, and examinees.

Sir William Osler said that in every department of human knowledge men were asking for guidance in the solution of a world-old problem—namely, how to train the mind and heart and hands of the young. Especially in connexion with medicine were men in a quandary, for there were new outlooks in every direction, not alone in the fundamentals of science and in methods of practice, but in the relations of the profession to the public and to the State. The chief difficulty lay in the extraordinary development in every subject of the curriculum—a new anatomy, a new physiology, a new pathology, new methods of practice, to say nothing of fundamental changes in physics, chemistry, and biology. Everywhere there was increased complexity and mind burdening terminology. What was the teacher to do, and, more important still, what was the poor student to do, when confronted with too much new knowledge and a Rabelaisian onomatomania? The diet of the modern student was extraordinarily complex and had far outrun an educational system which was framed in simpler days and to meet simpler conditions.

What a student knew and what he could do were judged by examinations, oral, written, and practical. Such tests had always loomed large in educational systems and might prove either their very essence or their ruin. The way in which examinations might come to control and sterilize the mind of a nation might be read in the story of China. The system of education in that country was not only more rigid and exacting than anything in Western life, but exercised a blighting influence owing to the fact that it was directed towards one single end—namely, the passing of examinations. The Greeks, on the other hand, taught that for the pursuit of knowledge to be productive it must be disinterested. Nothing could be more fatal to true intellectual training than constant preoccupation with its practical results.

As a subject of education, medicine had everything in its favour, since it made it an exceedingly easy task to carry out John Locke's primary canon in education—namely, to arouse interest. There was hardly a dry subject in the curriculum, yet a statement to this effect to an audience of medical students would arouse a smile. The student was early given the impression that he was at school to pass certain examinations, and the whole of his environment ground this impression into his soul. The atmosphere was Chinese, not Greek. Failure in teaching was admitted by the advertisement of special technical classes for the different examinations.

There was a second circumstance that proclaimed loudly how much out of touch with teaching were the tests imposed. Despite the fact that the qualifying examinations in the United Kingdom were well organized and admirably conducted, and that a man's knowledge was tested in the laboratory and by the bedside more freely than in any other country, the General Medical Council was forced year by year to issue a report which must demonstrate to any teacher beyond dispute how completely out of touch with the examiner either he or the student, or both, had got. A medical school was a human factory turning out doctors as the finished product at the end of five years' careful preparation and fitting of the mental machinery. Failure was incident to every human effort, and even the great motor-car shops turned out cars that were failures; but while these failures were few, the medical shops sent out medical motors which when turned out for trial on the road by the official experts were declared to be defective, and sent back to the shops in great numbers. Year after year the returns relating to the two great final subjects, which were the most attractive and the easiest to teach, showed rejections ranging between 35 and 46 per cent.

As far back as 1896 Mr. Pridgin Teale had introduced a motion dealing with this subject at a meeting of the General Medical Council, and had pleaded wisely and forcibly for reduction in the number of examinations, and for the substitution in certain subjects of a system of certification by the teachers and class examinations. Mr. Teale had remarked that with the multiplication of examinations the more fatal did they become, and the figures for the five years 1908–12 showed a continuation of the movement which Mr. Teale showed to exist when he spoke on the subject in 1896.

Sir William Osler, having quoted figures showing the rejections at examinations in the final subjects—medicine and surgery—held by the three Conjoint Boards and by the Universities of Edinburgh, Oxford, and Cambridge respectively for the five years ending 1912, then continued as follows: There is not so much difference, you notice, between what may be called the pass men of the Conjoint Boards and the men entering the universities, and I do not believe there is any special difference in stringency between the Oxford and Cambridge examinations and those of the London Conjoint Board. There are two other examinations which the *élite* of the student body affect. How do they stand? All regret that in London only the select and the elect attempt to get the degree of their own university. And it is difficult! Twice in the past five years more students have failed than have passed the final subjects for the M.B. The total figures for the period are: Of 1,061 candidates examined 481 were rejected, a percentage of 45.33. And, lastly, to one other qualification, greatly prized, sought only by the very best men, the Olympic athletes of their classes, I will refer — the F.R.C.S.Eng. Consider, please, how carefully this group is trained—only the very best venture to compete, and they have a diet of which the intellectual calories are gauged with surpassing accuracy. There is no doubt they are our very best, the picked steeplechasers of our stables. How do they fare? I am almost ashamed to read the figures. Your ears have tingled already, but only those hardened by familiarity will not be shocked at the demonstration of such a chasm between education and examination. Of 1,186 men who have tried for the primary Fellowship examination of the Royal College of Surgeons during the past five years only 365 have passed, 30.77 per cent. Of 680 men at the final Fellowship examination only 386 passed, 56.76 per cent. The high-water mark of examination futility was reached in May, 1912, when of 118 candidates for the primary Fellowship only 31 were approved. These are picked men, our very best students, the most carefully prepared, who rarely attempt the trial without months of extra study and attendance upon grinding classes. Of the ploughed I have known personally many seem to have been over-trained, others had spent their time in unprofitable original researches; but all, passed and plucked alike, I maintain, are of the highest type of our students, whose calamities proclaim to the world the breakdown of our present educational system.

The failure is general all along the line and in all grades—at the licensing boards, at the older universities of Oxford and Cambridge, at Dublin and Edinburgh, at South Kensington, and at Lincoln's Inn Fields—with singular uniformity all tell the same tale. There have been uneasiness and talk, but too much self-satisfied indifference, and even after the famous rout for the primary Fellowship in May last year I am told that satisfaction was expressed with the scope and method of the examination! Satisfactory to the examiners, perhaps, though I doubt it, but most unsatisfactory to the teachers, most painful to the students, and by no means a pleasure to the public as represented by the parents.

The speaker then put forward five suggestions as to the way in which relief from the condition of affairs

described might be obtained. In the first place, the curriculum should be simplified to give the students more time, and the teachers should have a freer hand in the matter of systematic lectures; the subject of medicine, for example, might be taught without any set lecture. Lectures had their value, and were precious when they came from the lips of a Watson or a Trousseau; but the day of the lecture had gone to give place to other methods better adapted to modern conditions. The saving of time if the lecture list were snipped in half or limited to a few subjects, such as physiology and pathology, would be enormous. The futility of attempting to teach all to all students should be acknowledged, and the anatomical fetish to which sacrifices had been paid too long should be burnt. Let them glance at Cunningham's *Anatomy*, for instance, a volume of 1,465 pages, many in small type. It was barbaric cruelty, when there was so much ahead, to burden the mind with minutiae which had only a Chinese value—a titanic test of memory. To schedule a minimum of the essentials should not be difficult once the great principle was acknowledged that in all departments of the curriculum only a few subjects could be mastered thoroughly. The secret of the tragic tale of rejections lay in a quotation which Socrates made to Alcibiades:

> Full many a thing he knew,
> But he knew them all badly.

Some universities made a practice of issuing schedules setting forth the minimum of essentials in different subjects.

Secondly, continued Sir William, relief may be obtained by giving credit for work done throughout the course, changing the present system of "signing up" for one of reports by demonstrators and assistants on the character of the work done by each student. Let all who teach examine. Let education and examination go hand in hand. Let the day's work tell from the moment a student enters the school. Everyone—from the junior demonstrator who supervises the student's first dissection to the professor—all should weigh while teaching. Day by day, as I see John Smith in the wards, and read his notes, and watch his clinical work and discuss the features of the patients, or as he narrates his case to the class about the bed and he and I have a Socratic dialogue, instruction and examination go hand in hand, and in such a way that at the end the formal tests should be but an amplification, an extension, and an inclusion of the scores of examinations which have been part of the routine of his life. Perhaps at present Utopian, this plan will be feasible in a new and reorganized generation, indeed it is feasible now in self-contained universities. Once accept the principle that instruction and examination should go hand in hand and the difficulty is solved. The returns are automatically passed on to the head of the department. Yes, but some one will say, "Take the judgement of a group of young teachers? It is absurd!" Not a bit. They see more of the students, come into closer contact, and are better able to judge of the quality of their work than the professor, and much more than any outside examiner. According to the character of his work a student should acquire much or little merit, and should be able to take to the examination table enough to pass, or at any rate to make the final test in any subject *pro forma*. Where the classes are small, as in many of the provincial universities, this plan could be easily worked. I have had practical experience of it and came to the conclusion that the judgement of the man who was fit to teach could be taken in estimating the progress of the student's education. And the system is being adopted. A few months ago I went into the beautiful clinical and pathological laboratory of the new Toronto General Hospital, and in one room I found an examination in pathology going on. The candidate had a set of cards in his hands on each of which were written the details of the *post-mortem* examination he had made with careful discussions of the cases. Pass or pluck really depended on the cards a man held. He brought his marks with him—instruction and examination had gone hand in hand. I was delighted to hear from Professor MacKenzie that the system, introduced at McGill by my pupil and successor, the late, much lamented Wyatt Johnston, had proved very successful in both Canadian schools.

Thirdly, simplify the examinations. Cut off some of the written papers. In the final subjects the long report on cases, bedside *viva*, supplemented if need be by a special "oral," will give examiners the necessary knowledge of a candidate's mental outlook. If they will consider not how much he knows, but how he knows what he knows, the long "written" is superfluous. As one watches a man handle a patient it is easy to tell whether or not he has had a proper training, and for this purpose fifteen minutes at the bedside are worth three hours at the desk. We must substitute for the quantitative estimate the qualitative, and judge the student as much by manner as by matter.

Fourthly, when possible, evidence of original work should be substituted for examination. Think of the stimulus to British surgery if, in place of the Egyptian tyranny to which our best students now slavishly bow, the President and Council of the Royal College of Surgeons selected for the Fellowship each year the fifteen or twenty of the men under 30 who had distinguished themselves most highly in surgical research. It would change the mental attitude of the younger generation, instil the spirit of Hunter into its members, and prevent the paralysing mental sterility that overtakes many good men who now spend precious plastic years in the dry drudgery of examination details.

Fifthly, compel no student to pass an examination twice in the same subject. At present brain and pocket alike suffer; and the burden could be lightened by a free reciprocity between the examining boards.

Speaking of examiners and their duties, Sir William said that men were usually very superior to the system which they worked, and this was true of examiners. He had found his fellow inquisitors, as a rule, the mildest mannered men that ever scuttled ships or cut throats. There were two extreme types—the metallic examiner who took a delight in finding out not what a man knew, but what he did not know, and the molluscoid examiner who was too soft-hearted to give any one less than 50 per cent. of full marks. But between these two extremes there was the large group of sensible examiners who tried to put the candidate at his ease and to find out what he knew in a simple, sympathetic manner. Nevertheless, whatever the type of an examiner might be, he was apt to take an unfair advantage of his position, and did so quite unconsciously. A specialist to whom the facts of his subject had become familiar and ingrained was apt to forget the years he had spent thereon, and might wonder when a man hesitated over an Argyll Robertson pupil or mistook a pericardial rub for an aortic insufficiency murmur. The most grievous mistake of the examiner was to regard the candidate as his mental equal, and to expect from him knowledge equal to that he himself possessed, ignoring his own long years of study and the short time into which the student had to cram acquisition of the knowledge of a dozen subjects. Examining was often a heart-breaking task, and it was much nicer to watch the gradual growth of a student's knowledge, and to get it out retail day by day, than to drag it out wholesale at set times. Much engaged men should not be chosen as examiners, and to examine in the evening after a hard day's work was to handicap the student. No doubt the time would come when professional examiners would be appointed by the General Medical Council to act as associates and assessors to the professors.

As for the examinee, the source of his troubles lay in intellectual dyspepsia from the cramming necessitated by the enormous development of the subjects that he had to take up. Defective preliminary education also helped to swell the rejections, though even among Cambridge men the percentage of rejections in some subjects was nearly 50 per cent. Weak men, fitted neither by breeding nor by early training to pursue medical studies, should be asked early to withdraw. It was infinitely kinder to stop a man in his career than to allow him to struggle on painfully and submit to the humiliation of half a dozen or more rejections. Finally, the speaker summed up his views as follows:

The student needs more time for quiet study, fewer classes, fewer lectures, and, above all, he needs the incubus of examinations lifted from his soul. To replace the Chinese by the Greek spirit would enable him to seek knowledge for itself, without a thought of the end, tested and taught day by day, the pupil and teacher working

together on the same lines, only one a little ahead of the other. This is the ideal towards which we should move. The pity of it all is that we should have made an intolerable burden of the study of one of the most attractive of the professions; but escape therefrom is in our own hands and should not be far off. A paragraph in an address of the late Dr. Stokes contains the pith of my remarks: "Let us emancipate the student, and give him time and opportunity for the cultivation of his mind, so that in his pupilage he shall not be a puppet in the hands of others, but rather a self-relying and reflecting being."

ANNUAL DINNER.

On the same evening the annual dinner of past and present students of St. George's Hospital Medical School was held at Princes' Restaurant. Mr. ADAMS FROST was in the chair, and in proposing the toast to the "Hospital and its School" made allusion to the approaching reunion with the Westminster Hospital, and gave an interesting summary of the events which led the two institutions to assume independent existences in 1733. A further consequence was the building of St. George's on the site of Lanesborough House, which then stood in the fields adjoining Hyde Park. In acknowledging the toast, Mr. WILLIAM WEST, the treasurer of the hospital, briefly outlined the arrangements that had been made for the amalgamation of the two institutions and of the general scheme of rebuilding which was in view. The precise details of the plan, as also the choice of a site, were still under consideration by a joint committee. The dean of the school, Dr. R. S. TREVOR, said that the school was still holding its own satisfactorily, and was very much alive, despite difficulties to which it was subjected in common with all medical schools in London. He made special appeal to old St. George's men not to allow a sentimental attachment to the present site to lead them to waver in their fealty to their old school when it had found a new home. The toast to past and present students and the St. George's Hospital Club, proposed by Dr. DRURY PENNINGTON, was acknowledged by Mr. IVOR BACK and Mr. GRELLIER; those to the orator of the day and to the chairman being proposed by Dr. ROLLESTON and Mr. G. R. TURNER respectively.

GUY'S HOSPITAL.

THERE was a slight variation in the 1st of October proceedings, which at this school usually take the form of a conversazione, preceded by an informal dinner, both being organized by the Students' Physical Society. This year, though the proceedings were still of a more or less informal character and plenty of opportunity was provided for exchanges of reminiscences between old friends, the conversazione was dropped and the evening spent in the college hall round the dinner table. Mr. COSMO BONSOR was in the chair, and many members of the staff were present. The principal after-dinner event was the report of the dean of the school, Dr. H. C. CAMERON. There had been, he said, extraordinary activity in every department of the school throughout the year. Not only had the new pathological department been brought into smooth working order, but the opening of the session was marked by the birth of three new departments — those of pharmacology, morbid histology, and chemical pathology. They were adequately housed, equipped, and staffed, and would do their work under the direction of Drs. A. J. Clark, G. W. Nicholson, and J. H. Ryffel respectively. With regard to the future, it would be folly to deny that the present time was a critical one for all medical schools in London, but Guy's, at any rate, had at least held its own. The number of students had even increased during the past year, and in regard to finances not a single scheme of improvement which had won general approval had had to be withdrawn, or even postponed, owing to lack of funds. All concerned had set themselves to perfect as far as possible the system of clinical training which was the pride of all the schools of medicine in London, and the report of the past year's work showed that the endeavour had by no means failed. His account of the dental school was equally satisfactory. The whole of the teaching therein had been reorganized, the number of students had very greatly increased, and the percentage of success obtained by them at recent examinations had been the subject of general comment among those interested in dental education. The same note was struck by Mr. COSMO BONSOR in proposing the toast to the medical and dental schools. During all the years that he had presided over business meetings relating to them the thing that had struck him most was, he said, the absolute determination both of the governors and of the staff that nothing whatever should be allowed to stand in the way of securing efficiency in teaching and full opportunities for investigation and research. They held that no financial sacrifice to attain these ends could be too great. Dr. HALE WHITE, who replied, said that both schools owed an enormous debt of gratitude to the long line of treasurers of Guy's Hospital, and especially to Mr. Cosmo Bonsor. Taking up the work at a time when the fortunes both of the hospital and of the school were depressed, he had steered them safely into their present fortunate position; the services that he had rendered to the schools could never be over-estimated. The work he had initiated was being admirably carried on by his successor, namely, the present treasurer, Viscount Goschen, who, to every one's regret, had been unable to be present that evening.

ST. BARTHOLOMEW'S HOSPITAL.

THE old students' dinner, which always marks the beginning of the winter session at this school, was held, as usual, in the Great Hall of the hospital on October 1st. The chair was taken by Dr. HOWARD TOOTH, now senior physician of the hospital, who in the course of the evening said that at no time in the school's history had its teaching machinery been more efficient. One index of development was the organization of separate departments under specialists. The former electrical department, for instance, was now subdivided, there being separate departments for *x*-ray diagnosis and electro-therapy, while the lecturer in pharmacology was now provided with a laboratory for research work. The proposal for the incorporation of the medical school was in abeyance pending consideration of the report of the Royal Commission on University Education in London. That report contained many important recommendations, but in regard to them he would at present only say that the school staff felt that the clinical teaching provided was the best in England, and that no amount of pictures and lectures could form an adequate substitute for actual contact with patients. He hoped, therefore, that whatever else might be done, that part, at any rate, of the present teaching system of the school would continue to be treasured. One effect on the hospital of the Insurance Act had been to reduce to a remarkable extent the number of patients in the casualty and out-patient departments, especially on the male side, because insured persons suffering from minor ailments were now referred to their own doctors. There was, on the other hand, little change in the number of in-patients, and the main opportunities for clinical teaching were not affected. He also mentioned the retirement from the staff of Mr. Bruce Clarke, Dr. West, Dr. Ormerod, and Sir Francis Champneys, and the deaths of Mr. Alfred Willett at the close of a useful and good life, and of Mr. Etherington-Smith, a surgeon of great promise. The memorial which the friends of the latter had decided to provide would take the form of refitting the old operating theatre and building over it a small ward for the treatment of members of the staff. This work, which would cost over £1,700, would shortly be completed. Mr. ACTON DAVIS, the acting treasurer of the hospital, replying to a toast thereto, also referred to the death of Etherington-Smith, and said that that sad event would recall to many of those present the name of James Shuter, a member of the staff who thirty years ago was cut off at the beginning of what promised to be an exceptionally brilliant career. He still hoped that the incorporation of the school would take place, as in his opinion the step would be of advantage both to the school and to the hospital. The latter had for a short time been bitterly abused for the interpretation which it placed on the Insurance Act; but since then the example set by it had been generally followed. All old St. Bartholomew's men should endeavour to dispel the idea that St. Bartholomew's was a rich institution and not in need of help, for its receipts from funded property were now at their zenith, and as the years went on were bound to fall. A permanent appeal department had now been formed, and

during the past year had succeeded in collecting over £8,000. The toast to the visitors was proposed by Dr. Herringham, who, speaking of the proposals with regard to the University of London, of which he is Vice-Chancellor, said that the report of the Royal Commission thereon certainly contained some mistakes and exaggerations, but contained likewise the germs of reforms which he hoped would obtain acceptance. This toast was acknowledged by Sir Pardey Lukis, Director-General of the Indian Medical Service, who said that one-seventh of his whole staff was composed of old St. Bartholomew's men, who also held about two-thirds of all the most important administrative and professorial appointments in the service. There had lately been some falling off in the number of candidates for admission to the service, but if this were due to lack of confidence in the service, he hoped to be able dispel it before his tenure of office was at an end. The formal proceedings of the evening ended with a toast to the Chairman, proposed by Sir Anthony Bowlby and acknowledged by Dr. Howard Tooth in a few happy phrases. The company then adjourned to the library.

ST. THOMAS'S HOSPITAL.

The customary gathering of St. Thomas's men on October 1st resulted once more in a very pleasant evening, bringing together, as it did, nearly 200 St. Thomas's students, past and present. Dr. H. B. Hawkins, now senior physician, was in the chair, and in the course of the evening supplied an interesting review of the recent past and immediate future of the hospital and its school. Both had ever proved ready to adopt new ideas and any changes which seemed likely to result in improvements; but as one looked back it was easy to perceive that a distinct shifting of the old line of advance had taken place. At one time it centred from the wards alone, then from both wards and laboratories in parallel lines, and now the laboratories were the leading position. Before they met again, however, next year the lines of advance were likely to be reunited, some working scheme by which ward work and laboratory work would be brought into closer contact having been devised. Laboratory work was becoming more and more essential in the diagnosis and treatment of disease; indeed, there was every reason to believe that the day of the general physician as at present known was passing away, since the temptation to specialize in some particular subject was becoming too strong to resist. Even in ward work a tendency in this direction seemed likely to make itself obvious, since hospital and school between them now possessed so many experts in various special branches of medicine. All this represented an immense change from the position of matters at the time when the speaker first joined the staff twenty-three years ago. Mr. Wainwright, the treasurer, likewise bore testimony to the great development that had taken place at St. Thomas's during his tenure of office. Recently there had been a considerable decrease in the number of ordinary out-patients, owing to the operation of the Insurance Act. A further by-product of that Act was the establishment at the hospital of a tuberculosis department, one of the objects of this step being to retain opportunities for instructing students in regard to tuberculosis and its treatment. In default of such a department, opportunities for this might have been lacking. Attached to the department was a ward to which cases could be admitted for the purpose of diagnosis, and whoever was in charge of that department had great opportunities before him. Further modifications of work might prove to be necessary as the result of the report of the Royal Commission on University Education in London. Some of the suggestions made therein seemed likely materially to interfere with the present organization of all the teaching hospitals in London, but for the present one could only wait and see. He had hopes that before October, 1914, the hospital would be possessed of a new out-patient department. One difficulty which stood in the way was that of choosing a site; possibly it would prove necessary to utilize some of the open spaces between the ward blocks, or the ground at present used as a cricket pitch. This would be regrettable, but obviously the needs of the hospital demanded the first consideration. Another speaker was the dean of the school, Dr. Stainer, who spoke of the success of the school in many directions, and of the excellent way in which it had been represented by old students on the battlefields of Eastern Europe during the recent Balkan wars. What the school chiefly needed now was encouragement in the direction of research work. The toast to the Chairman was proposed by Mr. E. F. Whyte, and the Chairman's acknowledgement brought the formal proceedings of the evening to a close.

CHARING CROSS HOSPITAL.

A large number of students and their friends assembled at Charing Cross Hospital on October 1st to witness the distribution of prizes to successful students of the medical school by the Lady Wantage, and to hear the report by Dr. William Hunter, the dean of the school, which was published last week. In the evening the past and present students reassembled round the dining-table, the outstanding feature of the after-dinner proceedings being the announcement that Dr. F. W. Mott had consented for the present to withhold his foreshadowed resignation of his position of senior physician of the hospital. The announcement was received with vociferous applause, which Dr. Mott acknowledged by supplying one of the excellent songs which for years past have formed one of the pleasant features of meetings of Charing Cross students. The dinner was held at Gatti's Restaurant, under the chairmanship of Mr. H. F. Waterhouse, who, in proposing the toast of "The Hospital and Medical School," said that the progress of the school had been continued and uninterrupted, and that it would be impossible to speak in too high terms of the reorganizing work undertaken by Dr. William Hunter as dean during the last two years. Dr. Hunter, in response, showed a modest tendency to shift the honours on to the shoulders of the vice-dean, Dr. W. J. Fenton. In response to a toast to certain non-medical guests, proposed by Dr. Eden, Dr. Burrows, the new Principal of King's College, said that the close connexion between Charing Cross and King's College, in the laboratories of which Charing Cross men undertook their preliminary studies, was, he said, keenly appreciated on both sides. The removal of King's College Hospital was also not without its effect upon the neighbouring hospital of Charing Cross, and with Westminster and St. George's going, he saw Charing Cross occupying an increasingly important strategical position in the hospital system of London. The Hon. Fletcher Moulton, son of the Lord Justice, also responded, as did Mr. Duff, chairman of the council of the hospital. The health of Mr. Waterhouse was proposed by Dr. J. Galloway, who delved into the mediaeval history of Charing Cross and its neighbourhood, and instituted a parallel between a certain Brother Lupus of the friary there—an angler, a good fellow, and a man who worked well with his colleagues—and their chairman of the evening.

ST. MARY'S HOSPITAL.

At St. Mary's Hospital there was a prize-giving ceremony on October 1st, when an address, of which some account was furnished last week, was delivered by Sir John Prescott Hewett. On the following day the school dinner was held at the Princes' Restaurant, and was very largely attended. Dr. Horace Swordер, of Luton, who was in the chair, proposed the toast of the evening, which was acknowledged, on behalf of the hospital, by Mr. Austen Leigh, chairman of the hospital board. Reviewing the events of the year, he mentioned as their outstanding features the rebuilding and enlargement of the casualty department and residents' quarters, the renewal of the drainage system of the hospital, and the completion of plans for the installation of a new mechanical lift. St. Mary's Hospital was one of the few institutions which might still be said to be situated just where it should be, and it was likely to gain increased importance from the migration of several other large institutions into the suburbs. Replying on behalf of the school, Sir John Broadbent, its dean, mentioned the more important successes of the year; they included the awarding of the Hungary prize to Sir Almroth Wright, of the Nettleship prize to Mr. George Coates, of the Jacksonian prize to Mr. F. W. Goyder, and of a gold medal in medicine to Dr. C. M. Wilson at the recent M.D. examination of the University of London. This made the eighth gold medal

obtained by the school during the past nine years. In addition, the only readers in the subjects of biology and chemistry at the University of London were both members of the school. The toast to "The Visitors," proposed by Dr. Graham Little, was acknowledged by Sir Arthur Conan Doyle and Mr. H. G. Handover; the latter, who is Mayor of Paddington, mentioned the increasing needs and size of the districts served by the hospital. Finally, Dr. Sidney Phillips, in proposin the health of the Chairman, drew attention to the strength in work and in athletics which had been brought to St. Mary's by Dr. Sworder and members of his family.

WESTMINSTER HOSPITAL SCHOOL.

A gathering of old students round the dinner table took place in connexion with the opening of the winter session at the Westminster Hospital School. Dr. Purves Stewart, after speaking of the future amalgamation of the institution with St. George's Hospital, suggested that an even larger problem awaited the consideration not only of themselves but of all those connected with great hospitals either in London or elsewhere, thanks to the ultimate effect of recent legislation. As patients with minor ailments were now going to panel doctors instead of to the hospitals, it seemed to him possible that the latter would be ultimately reduced to the condition which hospitals already occupied in Germany. Cases of tuberculosis would be taken out of their hands and treated at special institutions, and venereal diseases might eventually prove to be similarly ear-marked. Dr. de Havilland Hall, who also spoke at this dinner, said he was in favour of calling the future joint hospital the United Hospitals of St. George and Westminster. With regard to the site, he had originally been in favour of Clapham Common, but he now thought a site to the east of Wandsworth Bridge preferable, as there was space there for both out-patient and in-patient departments. The question of site, however, remained to be settled by the joint committee of the two hospitals.

KING'S COLLEGE HOSPITAL.

There was no formal ceremony in connexion with the opening of the winter session at King's College Hospital on October 1st, but on the following day a number of old students were conducted over the new buildings at Denmark Hill by the dean and other members of the staff of the school, and were afterwards entertained at tea. The same evening a large gathering of King's men met at dinner at the Waldorf Hotel, the chairman being Viscount Hambledon. Sir Watson Cheyne, in proposing the health of the Chairman, dwelt on the exceedingly satisfactory relations which existed between the management committee and the staff of the hospital, and mentioned that at no previous dinner of King's men had the chair been taken by a layman. On the present occasion, however, the eve of the opening of the new hospital at Denmark Hill, it was unanimously felt that an exception to the general rule should be made, and all present would join with him in expressing the hope that Viscount Hambledon would continue to be chairman of King's College Hospital for many years to come. Viscount Hambledon, in his reply, explained that the delay in completing the new hospital was largely due to the unfortunate disputes in the labour world during the last few months. He hoped, however, that in a few weeks the hospital would begin its work, and he was sure the staff would find the building in every way as perfect as it was possible to make it. He desired especially to thank Dr. Silk and members of the Watching Committee for the immense amount of time and labour they had given. To this would largely be due the perfection of arrangements in the new hospital.

UNIVERSITY COLLEGE MEDICAL SCHOOL.

At the dinner of past and present students of University College Medical School Mr. C. J. Bond of Leicester, who was in the chair, supplied an interesting account of the school as it was at the date when he was a student; and the dean, Dr. G. F. Blacker, stated that there had been a noteworthy increase in the number of new entries for the year. The endowment of the Students Bed had been completed, and the proposal to erect a squash racquets court in the gymnasium of the school had received considerable support. The proceedings ended with a toast to the Chairman, which was proposed by Sir Victor Horsley, F.R.S., and duly acknowledged.

BIRMINGHAM.

At Birmingham the opening of the new academic year was marked, as usual at this medical centre, by a conversazione given by the dean and other officers of the Faculty of Medicine. The principal event thereat was an interesting address by Professor Arthur Keith on the subject of present problems relating to the origin of modern races.

THE ROYAL VETERINARY COLLEGE.

Work for the winter session was formally inaugurated on October 1st by an address from Sir John McFadyean, who was able to give the students, new and old, a very satisfactory account of the prospects that awaited them when they had completed their studies and obtained their diplomas. They might rely upon their profession providing them with a comfortable livelihood. Though the growth of mechanical traction was diminishing the opportunities for veterinary work in regard to horses, this reduction was being counterbalanced by an extension of work in other directions. At the present time the veterinary profession was by no means overcrowded; either at home or in British colonies there were at least a dozen vacant veterinary appointments in the public services for which suitable candidates could not be found. As for the future, the Tuberculosis Order of the Board of Agriculture, which came into force last May, would provide the veterinary profession with a large amount of new work. Tuberculosis had an enormous hold on the stock of Great Britain, as of every other European country, but as a rule the incidence of the disease was out of all proportion to the loss that it occasioned. This loss would be lessened if due effect were given to the Order, but voluntary action on the part of each owner was necessary if tuberculosis were to be eradicated entirely from any single badly infected herd. Exceedingly few dairy herds were entirely free from tuberculosis, and few attempts to increase the number had hitherto been made, because of the inherent difficulty of the problem, and the absence of any prospect of adequate reward for the trouble and expense involved. Contagious abortion in cows was, from an economic point of view, now the most formidable disease of cattle existing in Great Britain. No one would deny that the bull might spread the infection, and for this reason the speaker regarded with misgiving the proposal recently made to endeavour to promote improved cattle breeding by subsidizing approved bulls for the service of cows belonging to different owners in the same district. If this plan were put into operation without any safeguard to prevent the spread of contagious abortion by the agency of the bull, very serious consequences would result. Another disease against which public action might have to be taken sooner or later was Johne's disease; it was usually spread by the sale of infected but apparently healthy animals in which the disease was known to exist. So long as that was permissible, its continued spread might confidently be expected.

THE SCHOOL OF PHARMACY.

At the School of Pharmacy, under the control of the Pharmaceutical Society, the beginning of the new academic year was marked by the distribution of prizes by Mr. E. White, the president of the society. The successful competitors included several women students. In his report the dean of the school, Professor Greenish, mentioned that the number of students was the same as in the previous two years—namely, 73, about as many as the school could accommodate. At the last examination 40 candidates had sat for the minor qualification, 31 passing; and 22 for the major qualification, of whom only 1 failed. The proceedings also included the presentation of the Hanbury medal to Dr. E. B. Power, Director of the Wellcome Chemical Research Laboratories. This medal, founded as a memorial to the late Daniel Hanbury, who entered the School of Pharmacy in 1844—that is to say, some three or four years after its first inception—is

awarded every two years for distinction in connexion with original research in the chemistry and natural history of drugs. Hanbury's chief work lay in the investigation of drugs from botanical and historical points of view, and of much of it Dr. Power gave a detailed account. In concluding his address, he remarked that, despite the immense fund of knowledge which had been accumulated in the course of the past centuries, the field of research was still practically unlimited. Even in only those branches of knowledge with which the School of Pharmacy was more particularly concerned there was much material ready to hand demanding investigation. Moreover, the vast continents of Asia and Africa, and the forests of South America, as well as the islands of the sea, doubtless contained among their natural resources many thousands of plants which had never yet been touched by hand, and a still larger number which had not so far been named or classified. There was no doubt that the chemical study of the immense number of plants or plant products whose constituents were as yet incompletely known would not only reveal much that was of scientific interest, but also medicinal principles which might prove capable of alleviating some of the ills of mankind.

"PROHIBITED IMMIGRANTS" TO AUSTRALIA.

The countries specially interested in the subject of immigration are giving increasing attention to the medical aspect of the problem. Excerpts from the instructions issued to Canadian medical immigration officers by the chief medical officer were recently published in the Journal. We are now informed that the Australian Commonwealth, at the close of last year, amended and extended its immigration legislation by defining more specially those liable to exclusion as "prohibited immigrants" on account of disease or physical or mental disability.

Classification of Prohibited Immigrants.

The following classes of persons are so defined under the Act:

(*b*) Any person not possessed of the prescribed certificate of health.

(*c*) Any idiot, imbecile, feeble-minded person, or epileptic.

(*d*) Any person suffering from a serious *transmissible* disease or defect.

(*e*) Any person suffering from pulmonary tuberculosis, trachoma, or any loathsome or dangerous communicable disease, either general or local.

(*f*) Any person suffering from any disease or mental or physical defect which is, in the opinion of an officer, liable to render the person concerned a charge upon the public or upon any public or charitable institution.

(*g*) Any person suffering from any other disease, disability, or disqualification which is prescribed.

The following diseases, disabilities, and disqualifications have been prescribed under (*g*).

Serious deformities or physical defects.
Insanity, an attack of insanity within five years of proposed emigration, or a history of two or more attacks of insanity or mental derangement at any time, or dementia.
Chronic alcoholism, senile decay.
Tuberculosis of the digestive tract, or the genito-urinary system, or of bones or joints.
Heart disease with signs of heart failure.
Chronic bronchitis with complications.
Paralysis or other serious nervous affections.
Cancer or other malignant growths.
Chronic rheumatism, rheumatoid arthritis, or rheumatic gout.
Severe hernia.

We understand that category (*d*) is intended to cover serious diseases and defects which may be transmitted by heredity, such as congenital cataract, deaf-mutism, or haemophilia, and that under (*e*) venereal disease would be included.

Necessity for Certificate of Health.

The most novel feature of this portion of the Act, however, is that requiring each immigrant to possess a prescribed certificate of health. This applies irrespective of the class in which the immigrant may travel, it being held that persons, except native-born Australians and those who are already Australian citizens, are immigrants if they are going to Australia to live there.

The prescribed medical certificate will not be obtainable from any medical practitioner, but only from approved medical referees, for the appointment of whom the Act provides. A Medical Bureau, under the direction of a chief medical officer, has been established in connexion with the High Commissioner's Office, and when all arrangements have been made all persons applying to shipping companies and agents for single passenger berths to Australia will be informed of the necessity of obtaining the prescribed certificate and be advised how to proceed in the matter. Dr. W. Perren Norris, formerly Director of Quarantine for the Commonwealth, has come to London to organize the Bureau, the address of which is 68, Victoria Street, Westminster, S.W.

The Commonwealth authorities consider that this provision in the Act will not only protect Australia, but will also prevent cases of hardship which arise under existing conditions whereby persons who have given up positions and sold up their homes may be rejected at the ship's side at the port of departure, or when they arrive at the port of entry in Australia. The burden now entailed on the shipping companies will also be reduced, as there should be few or no prohibited immigrants to be returned to Great Britain at the ship's expense.

It is held that the only humane course in dealing with any prospective emigrant is to afford facilities whereby before any expense is incurred it can be decided whether or not the person is in such a state of health or physical or mental condition as might lead to his rejection as an immigrant.

It has also to be borne in mind that under the Act the responsible Minister can make an exception and allow the admission of any person should circumstances warrant such action. In this way any undue hardship may, it is hoped, be avoided.

MOTOR CARS FOR MEDICAL MEN.

"Suburban" would be glad to hear the results of the experience of a doctor who has used a 10-h.p. four-cylinder Swift, light car, particularly as to its suitability for medical work, whether easily driven in traffic, and its consumption of petrol.

The Vauxhall Cars.

At a recent trial at Brooklands, a car manufactured by the Vauxhall Company achieved the task of keeping up a speed of 87.74 miles an hour for nearly eight hours in succession. The feat was the more notable because the chassis used was, except for being fitted with a racing body and having its gear-ratio and carburetter duly adjusted, a specimen of the ordinary light touring car sold by the firm under the title of the "Prince Henry" model. In addition to this model the firm is now building three others, which range from 16 to 35 h.p. The latter is a six-cylinder car, which moves with remarkable smoothness and absence of noise and vibration. The prices of the various chassis range from £395 to £625.

We are asked to state that until further notice the address of the Wellcome Bureau of Scientific Research, of which Dr. Andrew Balfour is director, will be 54a, Wigmore Street, London, W.

At the Italian Congress of Medical Radiology, which, as already announced, will be held in Milan next week, discussions will take place on the radiology of the cranium and of the intestines. There will also be a discussion on radium treatment in gynaecology, and visits will be paid to clinical radiology laboratories in the city.

A French translation by Professor Léon Guinet of the second edition of Dr. Charlton Bastian's work, *The Origin of Life*, has been published by H. Lamertin of Brussels. The article by Dr. Bastian entitled "Spontaneous Generation: its Reality and what it Implies," which appeared in the June number of the *English Review*, has also been translated into French, and is published in the *Revue Scientifique* for September 27th.

The Prussian Medical Ethical Court of Appeal (Ehrengerichtshof) has revised the verdict of the lower Ethical Court (Ehrengericht) in the case of a medical practitioner who was charged with advertising in a manner "derogatory to the honour and interests of the profession," by exposing notices concerning his sanatorium in railway carriages. The lower court had imposed a fine of £25, which the Court of Appeal reduced to £10.

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ARTHROPLASTY.

ARTHROPLASTY means the "restitution of functional activity in joints rendered useless by ankylosis," and it is no longer to be regarded as a procedure beyond the limits of the art of surgery. Tuffier over two years ago described to the Paris Surgical Society how in resection of joints he had in three cases successfully endeavoured to form a new joint by applying grafts of articular cartilage over the raw bone surfaces. It appeared to him that, as it was the property of cartilage to ensure mobility in joints, it was reasonable to use that tissue rather than any other. A few cases have been published by Lexer and others of successful transplanting of whole articulations, but these are so few that the method must be regarded as still in the experimental stage. Lorenz suggested the term "arthrolysis" in discussing the possibility of restoring mobility to a fixed joint. In his view everything depended upon the cause and form of anyklosis; while a tuberculous ankylosed joint ought to be left alone, it was safe to attack ankylosis after metastatic or traumatic suppuration, or following gonococcal infection, especially if the patient was young and generally in good condition. He advised that the best substance for interposing between the bones going to form the joint was a muscular or fascial flap cut from the immediate vicinity of the joint.

It is a little startling to find that in Mercy Hospital, Chicago, no fewer than 62 arthroplasties have been performed by Dr. John B. Murphy[1] and his assistants, in the hip-joint 16, the knee 28, the elbow 12, and others. Nearly all have reached the ideal set up by Dr. Murphy: restoration of the normal anatomical conformation of the joint, so that the patient shall have not only a movable but a strong and useful joint, fit to support the weight of the body, reliable enough to allow walking, running, and mounting stairs.

The theory upon which the operation is founded depends on the observation so often made that pseudarthrosis may result from interposition of muscle, fascia, or other tissue between the ends of fractured bone; in such a false joint intra-articular cartilage and membranes with a fluid resembling synovia have been found, and there was also a well-marked capsule. Many experiments along this line ultimately led to a definite technique, so that in 1902 Dr. Murphy was able to perform his first arthroplasty. Painstaking study elaborated many kinds of arthritis, and many varieties of ankylosis. The osseous, fibrous, and capsular types of ankylosis yield only to this operation, whereas the periarticular and extra-articular types may be treated successfully by such operations as capsulotomy and tendon lengthening. Osseous ankylosis only is dealt with in the paper. The author remarks that only in ankylosis following tuberculous disease is he fearful for the result, his apprehension being due to the indifferent reparative power of these patients and to the risk of stirring up dormant foci of the disease.

The main principle of the operation is to place between the ends of the bones, after their separation, and after their restoration as near as may be to the normal configuration, some material which will prevent recurrence of bony union. The best interposing material is a pedicled flap of fat and fascia lifted from the tissues in the neighbourhood of the joint, or, if that is impossible, a detached flap from the trochanteric bursal portion of the fascia lata may be transplanted. This interposing material must cover the entire articular surface of the bones, though fixed only to one. The technique of the operation in the hip may be outlined: A large U-shaped incision, base inwards, is made with trochanter well within the bay, and a flap of skin, cellular tissue, and fascia lata dissected up; the trochanter is sawn off and lifted away with its attached muscles; the femoral head is carefully separated from the acetabulum with chisels, and both surfaces rounded and smoothed; a flap of fascia lata from the trochanter region is split and folded over so as to form a complete covering for the head and neck of the femur; when the head is placed in the cavity of the acetabulum this flap serves as a lining for the cavity and reinforces the capsular ligament; the trochanter is fixed again in position by a nail. The leg is firmly extended and abducted. Passive movement is begun in seven to ten days. In three to four weeks splints are removed, and it is found that the patient has a fair amount of movement and may be encouraged to use the limb. In the knee the femoral condyles are rounded, corresponding concavities are formed in the head of the tibia, while the spine and intercondyloid ridge are preserved and refashioned so as to fit accurately into the intercondyloid notch. Two flaps of fascia lata are cut, one on each side, and stretched across the articular surface of the head of the tibia and sutured together. Passive and active movements are instituted, as in the hip. Methods similar to these are applied to the wrist and elbow.

The degree of success attained in Dr. Murphy's cases is so high that the operation may be said now to be quite within the sphere of practical surgery. Failures there have been from various causes—sometimes faults of technique, as insufficient interposition of fat and fascia, imperfect excision of capsule and ligaments; sometimes faults in after-treatment attributable to timidity and fearfulness in the patient. But probably failure left the patients no worse than before. The brilliance of the successes demonstrated by complete series of photographs and skiagrams in Dr. Murphy's paper more than counterbalances the misfortune of the few failures. Dr. Murphy is exceedingly cheerful in his outlook on the prospects of this operation. It is no longer an enthusiast's dream or a surgical curiosity. Perfectly movable joints with hinge and rotary motion of normal type have been produced. New synovialoid membrane is found with fluid not unlike synovial fluid, and lining membrane identical with that lining hygromata. These new joints support full weight and traction; they are painless once the process of repair is complete, and they are not subject to the haematogenous metastatic arthritis of other joints.

[1] Arthroplasty. By John B. Murphy, M.D., Chicago. *Annals of Surgery*, May, 1912.

INCOME TAX.

At this period of the year formal notice of the assessments for the financial year 1913-14 are being issued, and we would impress on practitioners generally the advisability of giving immediate attention to any communication they may receive with regard to the income tax.

This matter is of considerable importance, because the right of objection against an excessive assessment is extinguished by the failure on the part of the person assessed to give notice of objection within the time limited by statute. From cases which have been brought to our notice it appears that many persons are under the impression that nothing is lost by allowing an excessive assessment to stand, because it is believed repayment may be claimed at any time during the next three years. We would point out that a claim to repayment can be successfully made only when authorized by statute, and in general such authority can be claimed only in respect of insufficient allowances for abatement, life assurance, and children, and not in respect of an excessive assessment. In a repayment case argued in the King's Bench Division some years ago the court held that as the appellants had not appealed against the assessment made upon them they had neglected their proper remedy and could therefore obtain no redress, even upon a Petition of Right (Holborn Viaduct Land Co., Ltd., *v.* The Queen). The advisability of examining every notice of assessment immediately it is received is therefore sufficiently obvious.

In this connexion one or two remarks as to the points most likely to call for attention may not be out of place.

It is, of course, generally known that the claim to the "earned income" rate of duty must be lodged before September 30th each year, and that no subsequent claim can be admitted by the revenue authorities even on appeal. But where a practitioner has, before September 30th, intimated to the proper authority his intention of making the claim, but has not filed the full declaration of income which is required to complete it, the Board of Inland Revenue seems to be willing to regard that intimation as keeping alive the right of making a later and fuller claim; in other words, a bare statement of claim made before September 30th is allowed to be subsequently enlarged—by process of appeal, if necessary—into a complete claim.

For some time now the Board of Inland Revenue has waived the right of separately assessing under Schedule E the fees received by practitioners from public appointments; but the method of assessing the whole of the profits of the practice in one sum under Schedule D does not yet appear to be universal, and should a practitioner receive two or more notices of assessment, they ought to be closely scrutinized to avoid any double assessment of fees received from appointments.

We have before called attention to the claim of the income-tax authorities that the allowance for depreciation does not apply to professional earnings, but that the deduction of renewal expenses is held to be an available allowance. Accordingly, in any case in which a practitioner has renewed a car during the three years 1910-12, an appeal may be made unless the cost of renewal has already been deducted as a professional expense in arriving at the sum assessed.

Moneys received from Insurance Committees should be included as ordinary professional receipts in the assessment under Schedule D, and where previous assessments have been based on cash receipts the practitioner would, we conceive, be entitled to continue his return on that basis. That is to say, inasmuch as the fees payable through the Insurance Committees are in substitution for, rather than in addition to, previous receipts, the cash receipts for, say, 1912 may legitimately be taken in calculating the average liability, although there may be outstanding at the end of the year unpaid fees which have accrued due during that period.

Where practitioners are in partnership the notice of assessment will probably show only the total liability of the firm, but the surveyor of taxes will usually be prepared to supply on application details showing how the total liability should be shared between the partners. This apportionment should be examined and preserved, so that when the amount of income tax due is paid out of the firm's account, each partner may pay his share into that account. By this means each member of the firm will be certain of receiving the relief afforded to him by his own allowances for abatement, life assurance, etc.

The notice of assessment usually requires appellants to give notice in writing stating the grounds of appeal "within ten days of the date hereof" to the surveyor of taxes. The section which deals with this time limit (Section 57 (3) of the Taxes Management Act, 1880) is far from perspicuous, and we have known cases in which the period of ten days was not rigidly enforced. The dates on which the appeal meetings are held are stated on notices affixed to church doors (Section 119 of the Act of 1842), and though there may be some doubt as to whether appellants are entitled to more than the ten days' notice, the period certainly does not extend beyond the tenth day before the day of appeal.

The procedure to be adopted after the notice of objection has been lodged must, of course, vary according to the precise nature of the point involved, but in general an interview with the surveyor of taxes may be suggested as being likely to clear the ground, and possibly afford some opportunity for a settlement of the question by mutual consent.

Although the absence of any appeal generally makes the assessment binding, this is not so in some rather infrequent cases—for example, after a succession in a practice where the profits of the year of assessment have fallen short of the sum assessed, or when an actual loss has been incurred. In either of these cases a claim to repayment may be preferred at the end of the financial year, although no objection was taken to the original assessment.

THE DUCTLESS GLANDS AND THE NERVOUS SYSTEM.

The pathology of the ductless glands touches neurology at many points. Professor Murray, in the comprehensive review of the whole field of inquiry into the functions of those glands with which his Address in Medicine at Brighton was concerned, did not fail to deal with this division of the subject. But many readers of that address may be glad to have their attention directed to an excellent brief summary of our knowledge of the ductless glands, on the one hand, and of their connexions with the nervous system on the other, contributed to *Brain*[1] by Dr. T. R. Elliott. He points out that our knowledge of the ductless glands is

[1] Ductless Glands and the Nervous System. By Dr. T. R. Elliott. *Brain*, London, 1913, xxxv, Part 4, p. 306.

still very scanty, and has been hard to come by; the subject suffers from the encumbrance of various theories and hypotheses unsupported by observed facts, and he enters a strong plea for moderation and conservatism. There is great fear lest theory and analogy should be permitted to develop into statement and assertion, and be accepted as conclusive before their foundation on solid facts has been proved. As an example may be quoted the views of the Vienna school on the actual function of the thyroid secretion, as expounded by Falta and Rudinger. The thyroid and parathyroid secretions oppose one another, the former stimulating the sympathetic nerves, the latter inhibiting them. The secretions of the pancreas and of the parathyroids retard metabolism, those of the thyroid and adrenals augment it. Hence loss of the thyroid secretion leads to; (1) Slowed metabolism; (2) absence of stimulation of the adrenals; (3) escape of the pancreas from inhibition; (4) decreased excitability of the vagus in all its area of distribution, which includes the thyroid gland. This curiously intricate statement of function is purely speculative, as Dr. Elliott points out. To discuss it would be profitless, and the present need is the need of more facts. Medicine owes no gratitude to those who teach to her theories without proof.

One of the perplexing features of all these glands is their double anatomical constitution. In each two types of cell are found living in close neighbourhood, often under the same roof, and yet there is no obvious reason why they should have chosen such a union. Thus the thyroid gland is associated with the parathyroids. Removal of the latter has been supposed, though without sufficient proof as yet, to set up the condition of tetany by means of exaggerated excitability of the skeletal muscles through their motor nerves. The pituitary gland can be divided macroscopically into the posterior "pars nervosa" and the anterior glandular portion. Microscopically it is extremely complex, the anterior part containing two distinct types of cell, eosinophile and basophile (or chromophobe) respectively. The nervous part behind resembles neuroglia, tissue of an indeterminate character. Between the two lies the "pars intermedia" of the pituitary, roughly resembling the anterior lobe and consisting of columns of cells enclosing alveoli distended with colloid secretion. The adrenal glands are well known to consist of cortex and medulla; thanks to the work of the Cambridge School of Physiology, it is now recognized that the adrenal medulla consists of paraganglion cells, controlled by the splanchnic nerves and secreting the adrenalin that maintains the unstriped muscle regulated by the sympathetic nervous system at a fair level of tone for its routine activities in the blood vessels, viscera, and glands. The cortical cells do not belong immediately to the sympathetic group; overgrowth of the cortex in children is associated with sexual precocity and premature adolescence. As for the pancreas and the testis, though they of course are not ductless glands in the strict sense of the word, yet each produces an internal secretion, and each, again, contains secreting cells of two distinct types. The cortical cells of the adrenal glands secrete a special lipoid, a doubly-refracting fat, and so do the interstitial cells of the testis. It is also of interest to note that active chemical substances of relatively simple molecular constitution, not destroyed by boiling, can be extracted from the adrenal and pituitary glands, and, apparently, from the thyroid and the pancreas also.

Increasing emphasis has been laid in the last few years on the need to look upon the ductless glands as parts of a connected whole. If one is diseased, the others suffer, all having some functional relationship with each other. Careless clinical analysis is apt to accept this readily, concealing its want of exact knowledge beneath such terms as "pluriglandular insufficiency," "the polyglandular syndrome," "pauciglandular syndromes," "endocrine insufficiency," and the like. Dr. Elliott ends his paper with the enumeration of half a dozen common clinical features that suggest a common bond between the ductless glands, and urges once more the pressing need for further observation and research in the matter of these tangled relationships. He particularly commends the adrenals to the attention of any neurologists who may be seeking for deeper acquaintance with the physiological processes of nervous activity.

CANVASSING: A WARNING.

THE Association has recently received information to the effect that in several areas combinations of workmen or of friendly societies have been taking action which may seriously compromise those members of the profession who are parties to it. In several districts certain lay combinations, being dissatisfied with the terms on which the local medical profession is willing to undertake treatment of uninsured persons, have resolved to bring into the district some other medical man who will accept their terms. They have accordingly proceeded to advertise for a medical practitioner, and up to that point no legal question arises, though, of course, no medical man introduced into the district under such circumstances could expect professional recognition from those members of the profession whose standard of conditions and remuneration he was helping to undermine. But in several cases the friendly societies or workmen have gone further, and have made a public announcement that they intended to introduce a new medical man, and have then proceeded to canvass in order to induce insured persons to promise to transfer their names from the doctor on whose list they are at present to the practitioner it is proposed to introduce. There seems little doubt that such a course of action not only offends against all the traditions of the profession, but is a violation of the rulings of the General Medical Council in regard to canvassing and advertising. In some cases the practitioner who has been induced to accept such an appointment has proffered the excuse that he was not a party to the canvassing, and knew nothing whatever about it. Such an excuse is not adequate. Whether the advertising and canvassing were done prior to the engagement of the practitioner or not the fact remains that he profits by them. We would warn practitioners who are thinking of applying for appointments which are advertised by lay bodies of the kind mentioned to make sure that no canvassing or advertising of this kind has been done by the promoters of the scheme, and, if there is any doubt, to refuse to have anything to do with the appointment. The Association is collecting evidence in some of these cases, and intends to bring it to the notice of the General Medical Council. If those practitioners who respond to advertisements of the kind named find themselves charged before the Council with associating themselves with bodies which have canvassed for patients they will only have their own carelessness or indifference to blame. Members of the Association who are aware of such cases and can produce evidence in support of the charges of canvassing or advertising are urged to place themselves in communication with the Medical Secretary.

THE LEGAL RELATIONS OF INSURED PERSONS AND THEIR MEDICAL ATTENDANTS.

At page 968 will be found a record of a case which opens up the whole question of the relations, from a legal point of view, between a medical practitioner on the panel and a person he has accepted as one of his insured patients. The facts themselves are of interest, since they constitute a little vignette of some of the worries and inconveniences involved in the existence not alone of medical men who are on the panel, but of all general practitioners alike. No medical man, indeed, who has ever been in general practice can fail to sympathize with Dr. John Dunlop, the medical man concerned. But the facts in this case also show that if the judgement regarding them is allowed to stand these drawbacks of a medical life are likely to be materially increased in the case of panel doctors. A medical man in private practice, if he neglects, or declines, or delays to pay a visit, knows that the main risk run by him, if any, is that of losing a patient; but the judge of the Liverpool County Court appears to hold that in the same circumstances a medical man on a panel runs the additional risk of having to pay the fees of another medical man whom that patient may summon in his place. For the panel patient to succeed in imposing this expenditure on his doctor it would be necessary for him to prove not only that an adequate message had been received by his panel doctor and that the latter had failed to attend without due cause, but that the need of medical services was so urgent that there was real justification for asking another medical man to attend in his place. No doubt this would, commonly, not be easy, but nevertheless the possibility of success that this particular case suggests serves to justify the fears expressed in a letter which we have received from a correspondent. He believes that the judgement will pave the way for the growth of the belief that any panel patient who has a real or fancied grievance need not approach the Local Insurance Committee, but can at once serve a writ upon his doctor. As for the general effect of the judgement, it has been pointed out in this Journal on more than one occasion that the general relations between a medical man on the panel and a person whom he has accepted as a patient are those ordinarily existing between medical men and their patients. But the Liverpool judgement goes a good deal further. According to this, not only does an aggrieved panel patient stand possessed of the ordinary rights under the common law of an aggrieved patient of any kind, but he has special rights arising out of his contract. Furthermore, his contract is not with the Local Insurance Committee but with the medical man who has accepted him as one of his patients, such medical man, in addition to being liable for the results of malpraxis or professional negligence, as in the case of ordinary patients, being also liable to a penalty if he wrongfully omits to afford attendance of any kind. The judge appears to have held that Insurance Committees are merely agents, not principals, and that the framers of the National Insurance Act, as also the framers of the regulations of the National Insurance Commissioners, have omitted to introduce any clauses to control the legal relations between medical man on the panel and a person accepted by him as a patient. Both alike have merely provided for the settlement of such disputes as might be expected in ordinary life to end either by the medical man declining to place his services any longer at the patient's disposal, or by the patient informing his medical attendant that his services were no longer required. This being the case, the judgement becomes of such importance to all medical men on panels that a proposal made concerning it by the correspondent whose letter we have mentioned would seem to demand careful consideration. It is to the effect that a fund should be raised to enable Dr. Dunlop to take advantage of the permission to appeal granted him by the judge and contest the judgement in a higher court.

RESIGNATIONS IN SOUTH LONDON.

Many members of the Association who were distressed by the evidence of further weakness in the medical profession afforded by the defection from the British Medical Association of certain members in South London will have expected that the Journal would have the opportunity of publishing this week some better explanation of the course taken than was afforded by the speeches made at the meeting of the Wandsworth Division. They will find in the Supplement several letters on the subject, and among them communications from Dr. Edwin Smith and Dr. Barton, which, as they are written respectively by the mover and seconder of the resolution advising resignation, must be taken as the best defence that can be put forward. Neither letter, so far as we can discern, contains any new idea with regard either to the past or the future. None of the points put forward in the article published last week are seriously dealt with, and no suggestion whatever is made as to the policy which the Association should follow in order to safeguard the interests of its non-panel members. If to call a meeting and solemnly register the opinion that the Association is "reacting detrimentally to the interests of the members of the profession in general," and approving the action of members resigning, is not a course of conduct designed to injure the influence of the Association, all that can be said is that the Association may well pray to be saved from its friends. We have no intention of repeating what was said last week, but it may be well to say a word on one point to which Dr. Edwin Smith recurs, that is to say, what is described as the failure to appoint a non-panel committee. It is true that the Insurance Act Committee was asked to appoint a non-panel committee, but it must be obvious that, being itself a committee appointed jointly by the Representative Meeting and the Council, it had no power itself to appoint another committee, and that in fact, it could not do more than it did—namely, to appoint a Non-Panel Subcommittee. If that Subcommittee does not make its influence felt it can only be due to want of energy and courage on the part of non-panel practitioners. The experience of the Association, extending now over many years, of conflicts in which the profession has had to engage in order to safeguard its interests against the encroachments of Government departments and municipal authorities, proves that a policy which involves the comminution of the influence which a united profession can exert is essentially detrimental to the interests of members of the profession in general. Public opinion is disposed to be influenced when the profession is united, but it soon wearies when confronted with warring factions whose points of difference it does not fully understand. Dr. Napier Jones has sent us a short note, in which he describes the recent correspondence in the *Times* as "sorry reading. Whichever side," he adds, "is in the right, the only effect has been to hold the profession up to public derision." Dr. Cromie puts the general principle very well in the admirable letter published in the Supplement, and we would echo the appeal with which he concludes: "Let us credit those who differ from us with the pure motives we hold; let us cultivate the golden rule; let us be charitable to our neighbours; let us remember that there are two sides to every question; but, above all, let us reason and be reasonable, and with a united front we will weather the worst gales and ride safely under the shadow of the British Medical Association."

ABERTILLERY MEDICAL OFFICER OF HEALTH.

WHEN the Local Government Board advocated the employment of whole-time medical officers of health, and suggested that such a service might more readily be brought about in certain districts by adding to the duties of the medical officer of health those of school medical officer, isolation hospital superintendent, etc., it was confidently hoped that the Board would insist upon an adequate salary being paid to the holder of such a combined appointment. It is to be regretted that this anticipation has not been realized. The policy of the Board appears to be to require a net salary of at least £500 per annum for a whole-time officer, and this is the policy which the Association is striving after. In several instances, however, the Board has sanctioned a salary of £400, though making it a stipulation that the officer appointed should be permitted to earn fees from public health consulting work, the giving of expert evidence, etc. This course was adopted when Dr. A. E. Remmett Weaver was appointed medical officer of health to the Abertillery urban district in Monmouthshire. Dr. Weaver has now been elected, at a salary of £500, medical officer of health to the borough of Yeovil, and the Abertillery Council is seeking to appoint his successor on the same terms as those under which he held office. In view of the large amount of additional work which has been cast upon medical officers of health by recent legislation, both directly and indirectly, the Local Government Board is urging the payment of a salary of £500 per annum. The Abertillery District Council is composed very largely of colliery labour representatives, who would be the first to advocate for themselves and their fellow workmen the payment of a standard wage. They do not appear willing, however, to carry out this principle when it affects the medical profession, and are credited with the intention of refusing to acquiesce in the suggestions of the Local Government Board. It will be very regrettable if the Board sanctions the proposals of the District Council, for the arguments in favour of a salary of £500 for a whole-time medical officer of health are even stronger in the case of Abertillery than they might be in that of some other districts. The population of the Abertillery district approaches 40,000; it has an area of about ten square miles, and on account of its hilly character it is very difficult to work. Moreover, in addition to his ordinary public health duties, the medical officer of health acts as superintendent of the isolation hospital, he is school medical officer, and has the care of a school clinic. It would be difficult to find a better instance for a display of firmness on the part of the central authority.

COMPLEMENT DEVIATION IN GONORRHOEA.

IT is a curious fact that, while the complement deviation test was applied almost simultaneously to the diagnosis of syphilis and gonorrhoea, the nature of the reaction differs in important particulars in the two instances. Thus, the gonococcal complement fixation test discovered by Müller and Oppenheim,[1] is a true example of the Bordet-Gengou phenomenon, as the antigen is prepared from cultures of gonococci only, while the Wassermann reaction is not, for the antigen may be prepared from non-syphilitic tissues. It is therefore not remarkable that, if recent research has perhaps thrown some doubt on the value of the Wassermann test,[2] it has enhanced that of the complement fixation test for gonorrhoea. Schwartz and McNeil,[3] who have done important work in this subject, in their latest communications came to the conclusion that the complement fixation test for gonorrhoea is absolutely specific, and "a positive reaction denotes the presence or recent activity of a focus of living gonococci." In most cases the reaction does not become strongly positive till the fourth week of infection. If the disease is limited to the anterior urethra a positive reaction is not obtained, probably because the absorption of toxins is not sufficient to produce antibodies. The positive reaction does not entirely disappear till seven or eight weeks after apparent cure, owing to the persistence of antibodies in the blood; hence, if a strong positive reaction is obtained after this time it signifies that gonococci are still present although there may be no clinical signs of gonorrhoea. In 165 cases regarded as clinically cured Schwartz and McNeil found a positive reaction in 13 per cent. In women the reaction appears to be positive only when the disease has affected the cervix, but the test is of value in the gonorrhoeal complications of the female, owing to the difficulty of bacteriological diagnosis. In the differential diagnosis of gonorrhoeal arthritis from other forms the test is especially valuable. The above observers mention three cases of gout which gave a negative reaction, although in one of them there was previous gonorrhoeal infection on several occasions. Negative reactions were also obtained in arthritis deformans and in streptococcal and tuberculous arthritis. Of great interest were cases of syphilitic arthritis, which gave a negative gonococcal reaction, but a positive Wassermann, thus showing that the test allows of specific differentiation, even in the presence of another disease, such as syphilis, which also causes deviation of complement. A negative reaction does not absolutely exclude gonococcal infection, but, according to Schwartz and McNeil, it is of much more value than a negative Wassermann reaction in the case of syphilis, for the latter may depend upon recent mercurial treatment, or upon the encapsulation of spirochaetes by a connective tissue capsule which hinders the absorption of toxins, and consequently the formation of antibodies. Comparing the relative diagnostic value of the complement fixation test and bacteriological diagnosis, these observers remark that bacteriological proof of the presence of the gonococcus is not so easy to obtain, owing to sources of error, such as degeneration forms of Gram-positive cocci which do not retain the Gram stain, the *Trichomonas vaginalis*, aberrant forms of the *Bacillus coli*, and the *Micrococcus catarrhalis*. Thus, bacteriological diagnosis depends on culture, the technique of which is more difficult than that of the complement fixation test. The gonococcus grows only on special media, and culture may fail owing to the presence of other rapidly growing organisms which inhibit the growth of the gonococcus or render its isolation impossible. They therefore conclude that the complement fixation test gives more reliable results than bacteriological methods in the majority of doubtful cases. They also draw attention to the use of the test in medico-legal cases, for the absence of a positive reaction in the early stages of infection may distinguish between a fresh infection and the recurrence of an old one. This has an obvious application to cases in which symptoms of gonorrhoea appear after marriage.

[1] *Wien. klin. Woch.*, No. 19, 1906.
[2] BRITISH MEDICAL JOURNAL, May 17th, 1913.
[3] *Amer. Journ. Med. Sci.*, September and December, 1912.

THE SURGICAL TREATMENT OF PULMONARY TUBERCULOSIS.

THE possibility of affording temporary relief to cases of advanced unilateral consumption by the artificial production of pneumothorax has been but little regarded in this country. The chief English textbooks contain few references to it, but successful cases are recorded by French and American authors, some of whom go so far as to assert that the method is "now adopted by most of the leading sanatoriums and hospitals for diseases of the lungs on the Continent and in the United States." Statistical evidence as to the proportion of success achieved is not as yet forthcoming. In a critical article on the whole question,[1] Dr. Louis Rénon examines the theory upon

[1] *Journal des praticiens*, January, 1913.

which the treatment is founded, and reviews the numerous points of difficulty with which the practical application of it is beset. Theoretically the provision of physiological rest to the damaged lung is a sound therapeutic proceeding. In practice, however, the uncertainty which must attend the diagnosis of the exact condition of the affected lung and pleura renders the choice of suitable cases extremely limited, and even with the exercise of the greatest caution the operation has sometimes tended to shorten life rather than to prolong it. The best result that can be hoped for is a postponement of the fatal day, and it would appear that this has sometimes been attained for considerable periods, during which the life of the patient has been rendered less miserable than before. The idea seems to have been first suggested in 1892 by Forlanini, and three years later a series of original observations were published by J. B. Murphy. Dr. Pearson read a paper on the subject at the annual meeting of the British Medical Association last year,[2] supplementing the description of the procedure recently contributed by him and Dr. Lillingston to the first volume of the *System of Treatment*, edited by Drs. Latham and English. The operation of introducing gas into the pleural cavity is obviously restricted to cases of unilateral disease, but it is claimed that large apical cavities have been collapsed and cicatrized, and that the rest of the lung has then been allowed to expand and has come into effective use again. The method of introduction is elaborate, nitrogen gas being employed at several sittings, the treatment sometimes continuing for long periods in order to maintain the collapse of the lung and overcome its tendency to re-expand prematurely. The closed method by means of a needle is regarded as the safest, although Murphy obtained success by the open plan of cutting down upon the parietal pleura in the first instance. The possibility of setting up purulent effusion has always to be borne in mind, and serious disturbance of cardiac action has been known to occur. The latter days of the patient with advanced disease carry little but misery to the patient himself and depression, if not danger, to those about him, and the possibility of affording temporary relief and cessation of the constant output of infective sputum is worthy of consideration. "Hundreds of cases," we are told, have already undergone this line of treatment, but it is to be feared that the percentage of advanced cases with disease limited to one side, and with a pleura sufficiently detached to admit of inflation, is only to be counted in units.

MEDICAL TERMS IN THE NEW ENGLISH DICTIONARY.

The current part of the *New English* (or Oxford) *Dictionary* passes in alphabetic sequence from *Tombal* to *Trahysh*, two words which, it can be imagined, are hardly familiar to every reader, but yet are not so difficult when the key is provided, for *tombal* simply means pertaining to a tomb, and *trahysh* is to betray. Neither the first nor the last term in the part, therefore, is medical. There are, however, crowds of truly medical words in this section of Sir James Murray's great work, and it is difficult to choose from the number those of most interest. If we take the words derived from the Latin *toxicum*, which in turn is an adaptation of the Greek τοξικὸν φάρμακον (poison for smearing arrows), we shall probably have enough to illustrate both the method of the *Dictionary* and the fullness of the present part of it in medical expressions; there are between fifty and sixty terms coming from *toxicum!* Most of them, too, are products of the past twenty or thirty years and some are only of yesterday. Every one knows, of course, what *toxaemia* means (blood poisoning), but hardly any one, perhaps, is aware till they read it in the *Dictionary* that it has an anglicized form—*toxemy*. *Toxalbumin*, or *toxalbumen* (both are right, but Sir James Murray puts the *in* first), means a poisonous or pathogenic albumin or protein produced by bacteria, or, shortly, a protein toxin; *toxalbumose* is a poisonous albumose, *toxamine* is a poisonous amine, and *toxanaemia* is anaemia caused by the action of a poison. It is significant that all these words except the first are illustrated by quotations which go no further back than the beginning of the present century or the last decade of the nineteenth. Another batch of equally modern terms contains such words as *toxidermic* (pertaining to skin disease produced by a poison), *toxignomic* (enabling one to diagnose the action of a toxin), *toxihaemia* (equivalent to toxaemia), *toxi-infective* (characterized by infection due to a toxin), *toxiphagus* (one who eats poisons), *toxiphobia* (fear of being poisoned, as a form of insanity or monomania), and *toxiresin* the name of a poisonous substance obtained from digitalis by the action of acids. The word *toxic*, however, is much older, and Sir James Murray has captured an illustrative quotation from the seventeenth century, from Evelyn's *Sylva*, published in 1664 ("the toxic quality was certainly in the liquor . . . not in the nature of the wood"); but he has to go forward to 1876 to get a second instance of its use. *Toxical* was also used in the early years of the seventeenth century, and as for *toxicate* (poisoned or poisonous) there is a quotation from about 1470, and the word is now obsolete. *Toxication*, however, is moderately modern (1821); it means poisoning especially by toxic substances produced by disease germs; and *toxicity* is quite modern (earliest use was in 1881), and signifies toxic or poisonous quality, especially in relation to its degree or strength. Another large group of words begin with *toxico-* and include *toxicaemia*, *toxicoderma*, *toxicodermatitis*, *toxicodermitis*, *toxicogenic*, *toxicohaemia*, *toxicomania*, *toxicophagous*, *toxicophagy*, *toxicophobia*, *toxicotraumatic*, as well as the familiar *toxicology* (the science of poisons) with its derivatives (*toxicologist*, etc.). The *toxicoses* are a family of diseases caused by the reception of poisons into the system. Then there is *toxin* (a specific poison, usually of an albuminous nature, especially one produced by a microbe, which causes a particular disease when present in the system of a human or animal body) and *toxinaemia* (the presence of a toxin in the blood). A group of extremely new, almost neonatal, words begin with *toxo-*, and include *toxo-infectious*, *toxo-peptone* (or pepto-toxin), *toxophil* (having affinity for a toxin), *toxophore*, *toxophoric*, or *toxophorous* (poison-bearing), *toxophylaxin* and *toxozosin* (defensive proteins or antitoxins), and *toxoprotein* (a toxic protein or mixture of a toxin and a protein). Finally, there is *toxoid* (a modification, or transformation product of a toxin, in which the toxophoric group of atoms is lost, and which has, therefore, no toxic effect, but retains affinity for the antitoxin), *toxon*, and *toxonoid*. Along with, and mixed with, these poison-words are several terms, such as *toxophilite*, *toxophily*, and *toxology*, which mean a lover of the bow and the practice and study of archery. How come they *dans cette galère?* The answer is simple enough: the word *toxicum* was the Latin for poison, but the full term was the Greek τοξικὸν φάρμακον, poison for smearing arrows, and only the first word was carried over, and, as it chanced, it meant, not poison, but arrows, from τόξον, a bow. As Sir James Murray puts it, the effect of using *toxicum* as short for the Greek phrase was to transfer the sense "poison" from φαρμακον to *toxicum*, first as "poison for arrows," and at length as "poison" generally. There is no space for notes of the other medical terms (such as *tonsils* and their derivatives *trachea*, *topical*, *tormina*, *tractoration*, *tourniquet*, *tragacanth*, *tragus*, etc.), but the *toxi-* group will have given some idea of the extraordinary fullness and accuracy of this magnificent lexicographic undertaking.

[2] British Medical Journal, 1912, vol. ii, p. 957.

PLETHORA ABDOMINALIS.

A symptom-complex has been described by Julius Shiötz under the old title "plethora abdominalis." He traces it to primary disorders of the splanchnic circulatory system, with venous stasis in the abdominal organs. A typical patient is a man, 40 to 50 years old, who complains of a sense of pressure and fullness in the abdomen, constipation, flatulence, tiredness, pyrosis, giddiness, palpitation of the heart, and a feeling of anxiety. While the arms and legs are emaciated, and the chest is flat, the belly is distended, and the complexion muddy. When examined in the upright position, the heart appears dilated, the cardiac dullness being increased, notably to the right. The blood pressure is often high, and the urine contains albumen and granular casts. Often, too, the liver is somewhat enlarged, and it may even be tender. In the axilla, at the level of the seventh and eighth ribs, signs of atelectasis may be detected. Many other symptoms are those of neurasthenia, enteroptosis, gastric atony, and hyperacidity. Fatty degeneration of the heart may also be diagnosed, but in plethora abdominalis, with change from the erect to the horizonal position, the signs of cardiac dilatation disappear. This is due to the fact that, while meteorism in the erect position displaces the diaphragm upwards, and thus presses the heart against the anterior chest wall, the horizontal position renders the abdomen flaccid and the intra-abdominal pressure less high. The heart, in consequence, is no longer displaced, and its boundaries appear normal. The patient's occupation is usually sedentary, and his most important troubles, such as distension of the abdomen, enlargement of the liver, haemorrhoids, and varicose veins of the legs, are all traceable to stasis within the abdominal blood vessels. The signs and symptoms referred to the heart are, however, secondary; and are due to the meteorism. If general adiposity or fatty degeneration of the heart be diagnosed, a dietary suitable for such conditions will be prescribed. This may, indeed, lighten the heart's work, but it will not lessen the obstruction to the circulation in the abdomen. If, however, primary venous stasis of the abdomen be recognized, treatment will include the systematic use of aperients, abdominal massage, gymnastic exercises, a non-fermentative diet, and air and carbonic acid baths.

WANTED—A HOSPITAL FOR ANIMALS.

At an inaugural lecture delivered a short time ago before the University of Liverpool, Professor Glynn[1] put forward strong arguments for the establishment of a complete and up to date animal hospital in connexion with the local veterinary school, supporting his arguments with a mass of evidence as to the achievements of veterinary science in the prophylaxis and treatment of animal diseases that merits the most serious consideration. Animals have been the servants and the friends of man since, perhaps, the neolithic age. As a result of this long association there are now a great many diseases common to both man and animals, and it is undeniable that if these diseases could be studied in animals as thoroughly and as scientifically as they can be studied in man, it would be greatly to the advantage of both. We shall not attempt to reproduce the full list given by Professor Glynn of the infective diseases of animals that may be communicated to man; a few only can be mentioned. Tuberculosis naturally occupies the first place here. The Royal Commission on Tuberculosis has recently published its conclusions, and we are to believe that the tubercle bacilli found in cattle and in man respectively are two different varieties of the same bacillus, and that the disease they produce is the same disease; bovine tuberculosis does infect man, and is responsible for a large amount of tuberculosis in children, which is usually contracted by drinking cow's milk. The Commission has urged the Government to take steps to eliminate tuberculosis from the domestic animals, and so reduce the amount of human tuberculosis; in 1910 over 51,000 persons died of tuberculosis in England and Wales, while Delépine estimates that there are not less than 1,600,000 tuberculous cattle in Great Britain alone. Tetanus, fortunately rare in man, is comparatively common amongst horses; the tetanus bacillus is a normal inhabitant of the intestine of many herbivora, and so is particularly common in manured soil. Anthrax is a most deadly disease among cattle and sheep, and very prevalent in certain parts of Europe and Asia; 908 separate outbreaks occurred amongst the cattle of Great Britain in 1911. The reasons for its spread and its frequent appearance in farms where there has been no previous case are not yet understood, and urgently demand investigation; it is practically always fatal to the animals it attacks, and a number of fatal cases occur every year in man. Rabies or hydrophobia has not been seen in Great Britain since 1903; this triumph was due to a general muzzling order enforced in spite of the strenuous opposition of those who maintain that a "compulsory collar order" would have been equally successful. Rabies still occurs in Germany, where a "collar order" is in force. Plague in its bubonic form is dependent on the infection of rats, the disease being spread from rat to rat, and from rat to man, by the bites of rat-fleas; fortunately the rat-fleas do not often bite man. Sleeping sickness, the scourge of equatorial Africa, threatens to prevent the civilization and even the inhabitation of great tracts of territory there; it has recently been found that a large proportion of the wild game in equatorial Africa is infected with the trypanosomes of sleeping sickness, and acts as their carrier. Though apparently quite healthy, the wild beasts infect the tsetse flies that feed upon them, and the tsetse flies in turn infect man. Want of space prevents more than a mention being made of the many other diseases common to animals and man—glanders, Malta fever, foot and mouth disease, the swine fevers (at least three are distinguished, and one occurs in man), cerebro-spinal meningitis, probably acute poliomyelitis, various types of acute food and milk poisoning, rat-bite fever, and other rare or exotic disorders. Professor Glynn goes on to consider a number of the infective diseases of animals that are apparently not communicable to man, and finally comes to the important matter of the present undermanning of the veterinary profession. During the five years—1907–1911—the average number of men entering the veterinary profession was but 85 per annum, as compared with an average annual entry of 140 during the preceding five years. This shortage he attributes to two main factors—want of appreciation by the public of the services that can be rendered by the veterinary surgeon, and want of proper training and facilities for his education. The Government recently appointed a Departmental Committee to inquire into the matter, realizing its importance, and its report has now been issued, with recommendations for increasing the output of veterinary surgeons and raising the standard of their training. To give an example of the low ebb to which this country is now reduced, the fact may be mentioned that there are only about 50 whole-time veterinary inspectors of our meat supply, while the United States have 775 and Germany 1,180. But it must be admitted in extenuation of these apparently alarming figures that in this country we indulge but little in the practice of eating raw meat, so that trichinae and the pathogenic bacteria of all sorts that go with meat are in greater danger of death by cooking and are less likely to live to infect the inhabitants of these islands than is the

[1] *The Study of Disease in the Domesticated Animals, its Importance to the Community, with a Plea for an Animal Hospital.* An inaugural lecture by E. Glynn, M.A., M.D.Cantab., M.R.C.P.Lond. Liverpool: University Press. 1913. (8vo, pp. 40. 1s.)

case in some other countries. Professor Glynn concludes his most interesting and instructive lecture with a strong appeal to the University of Liverpool to found a hospital for the study and treatment of disease in the domestic animals, and with an outline of the requirements and management of such an institution. His lecture places on record the vast amount of success that has already followed the work of veterinary medicine and surgery in the prevention and treatment of the diseases of animals, whether common to man or not, and it indicates the countless lines upon which similarly productive research will follow in the future. He makes out a very strong case, and we hope that his appeal will not fall upon deaf ears.

LAPAROSCOPY.

Following the discovery of the endoscope, which has revealed the secrets of the open cavities of the body, the stomach, bladder, and rectum, suggestions for the exploration of the closed cavities by similar means have been made. Jacobeus of Stockholm has laid down rules for the practice of laparoscopy and thoracoscopy; and Rénon[1] has recently described the method in some detail. Briefly, it consists in passing the cystoscope of Nitze along the hollow sheath of a large trocar into the abdominal cavity. The cystoscope used is one of small diameter, terminating in a metallic filament lamp supplied by a current of 3 volts and 0.02 ampères. The trocar sheath terminates in a small chamber provided with a metallic valve, by means of which communication between the peritoneal cavity and the air can be shut off when the sheath is empty. The instrument is sterilized before use, the cystoscope itself being kept in the vapour of formol for twenty-four hours. The technique varies according to the presence or absence of fluid in the abdominal cavity. In the former case it is comparatively simple. The abdomen having been entered under local anaesthesia the fluid is evacuated, a smaller sheath having been first introduced within the large one to keep the valve open. Air or oxygen is then introduced after sterilization by filtration through cotton wool, and the abdominal cavity is moderately distended. The inner sheath is then withdrawn, and the cystoscope introduced. When the examination has been completed the light is extinguished a minute or two before withdrawal. This having been done, the inner sheath is again passed, the valve kept open, and the air evacuated. If there is any therapeutic purpose in view, the air may be quite safely left in the abdominal cavity. When no ascites is present a modification of the technique is necessary owing to the risk of injuring the intestine. Rénon is of opinion that the method of Jacobeus in these circumstances is not free from danger, and that it is by no means easy. After an exploratory incision under local anaesthesia, he prefers the use of the ingenious instrument invented by Küss to penetrate the pleural sac in the formation of an artificial pneumothorax. The variations of abdominal pressure are noted by a water manometer. Once in the abdominal cavity air is insufflated, and the large trocar sheath with the cystoscope, is substituted for the trocar of Küss. Jacobeus has in this way successfully established the diagnosis of cirrhosis of the liver, various tumours of the abdomen, and several cases of tuberculous peritonitis. The characteristics of the various diseases were clearly demonstrated *in situ*, and every detail was visible, notably in the cases of tuberculous peritonitis. Rénon asks the question whether the use of laparoscopy would be justified in subacute and chronic appendicitis or in colitis, and suggests that this will only be so when the technique necessary in non-ascitic cases has been further perfected. When this has been done he believes there will be great scope for this method of diagnosis.

[1] *Journal des praticiens*, March 8th, 1913.

MUSEUM DEMONSTRATIONS.

The autumn course of demonstrations given in connexion with the museum of the Royal College of Surgeons of England will commence on Friday next, October 17th, at 5 o'clock, when Professor Keith will exhibit specimens illustrating various forms of defective bone growth. In this demonstration the condition described recently by Porak and Durante, under the name of *Dystrophie periostale*, will be described and exemplified by two specimens which have been added recently to the College collection. Although periostale dystrophy, in its clinical manifestations, has certain resemblances to achondroplasia, pathologically it must be regarded as a totally distinct disease. At subsequent demonstrations, which will be duly noticed in our Diary of the Week, Professor Keith will exhibit specimens showing the anatomical and pathological characters of the great bowel in cases of intestinal stasis, and a group of specimens which illustrate the anatomy of the internal ear. The series of demonstrations given by the Pathological Curator of the Museum, Mr. Shattock, will cover the collections of tumours of benign origin—including fibroma, chondroma, and myxoma—as well as the section of the museum which shows inflammatory changes in bone. Mr. J. F. Colyer, Honorary Curator of the Odontological Collection, will give two demonstrations on irregularities of the teeth. The demonstrations are free to all medical men and to students working for higher examinations in surgery.

THE CINEMATOGRAPH IN PHYSIOLOGY.

In the brief account which was given in the Journal of September 20th of the International Congress of Physiologists, held in Groningen at the beginning of September, reference was made to some cinematograph films for the analysis of movement, shown by Professor Richet for Dr. L. Bull of Paris. The films were run at such a pace that the movements thrown on the screen seemed to have been very slowly performed. In the course of a demonstration to medical practitioners and students on some recent applications of cinematography to physiology, given in the Medical School of the University of Manchester on October 3rd, Professor Stirling showed several films taken on this principle. He explained that the images were taken at the rate of 180 a second and projected in the ordinary manner, namely, at the rate of 16 a second. He also showed, in the course of his very interesting demonstration, a series of films of microscopic objects taken by the dark ground method of illumination. Among other phenomena illustrated by the films was the circulatory movements of the protoplasmic and chlorophyll granules of water plants, the growth of the protoplasm of slime fungi at the circumference, the circulation of the blood in small crustaceans, and the movements of various species of spirochaetes. Professor Stirling also demonstrated the growth of fragments of embryonic tissues removed from the living body and maintained in a culture chamber. Among the objects shown to illustrate these points were fragments taken from the spleen and from the heart of the embryo chick, and others in which cell division could actually be observed in progress.

H.R.H. Princess Louise Duchess of Argyll will open the Mothers' Hospital at Clapton, N.E., on Saturday, October 13th.

Dr. R. C. Brown, whose readiness generously to assist worthy educational causes has been so often shown, has recently presented an organ to Preston Grammar School, at which he was educated. The President of St. John's College, Oxford, who was present on the occasion, referred to the importance of music as a branch of school life and school work. The attitude of teachers and boys to it had undergone a salutary change, and in this materialistic age music was by no means the least of the arts that ought to be taught to boys.

Scotland.

[From our special correspondents.]

Medical Education in Glasgow.

A clinical research laboratory erected within the grounds of the Victoria Infirmary, Glasgow, was opened on October 2nd. It is the gift of Mr. William Robertson, shipowner, and at a meeting held immediately before the opening ceremony—which was performed by the donor—Lord Rowallan, the chairman of the governors, said that all experts agreed that a clinical laboratory was a necessary adjunct to a hospital. He believed that hospitals would find no difficulty in getting all the money they wanted if the public had more imagination; if it required imagination to sympathize with the ordinary work of a hospital—the direct relief of suffering—a still more vivid imagination was required to realize the necessity for a clinical laboratory, and the greatest credit and gratitude was due to Mr. Robertson for having come so generously to the assistance of the Victoria Infirmary in this respect. Mr. William Robertson said that his gift had its origin in a conversation with Dr. Ebenezer Duncan. In realizing the project to erect a clinical laboratory for the Victoria Infirmary Dr. Mackintosh and Dr. Grant Andrew had given invaluable assistance, and now that the laboratory was established he hoped that it would help to keep the Victoria Infirmary in the forefront of clinical knowledge, and be a power for good to the community. Dr. Donald Mackintosh, M.V.O., thanked Mr. Robertson for his benefactions to that and similar institutions, and congratulated the governors and staff of the Victoria Infirmary on the possession of a building not surpassed by any in the United Kingdom, and equipped with the best that money could purchase. He looked forward hopefully and confidently to much original work being done within the building, for the laboratories would facilitate the labours of the staff and furnish precise data enabling them not only to diagnose cases with greater ease and accuracy but to treat them with greater confidence. The under part of the laboratory building would be used by the attendant in preparing various media, and in fact would be the workshop for the laboratory. In this part of the building there also was an excellent lecture room where scientific meetings might be held. On the flat above was the laboratory proper, which he hoped would shortly have an entrance more in keeping with the building. He was led to believe that the governors had in view the extension of the main corridor which led to the upper part of the building where the scientific work would be carried on. Here there was a large clinical laboratory where quite a number of medical men might work together and have the advantage of each other's counsel, in addition to the assistance and guidance of the director. Adjoining this large clinical laboratory there was a chemical laboratory. The finest balance which could be purchased had been placed in a room specially devised to obviate vibration, and a special vaccine room and a fully equipped sterilizing room and an incubator room had also been provided. A vote of thanks to Dr. Mackintosh, Dr. Duncan, and Dr. Grant Andrew for their services in connexion with the erection and equipment of the laboratory was adopted, and Dr. Duncan made a suitable reply.

The late Mr. William Weir of Kildonan left several large bequests to institutions in Glasgow, including a bequest of £5,000 to the university, the income of which is to be applied in providing an additional assistant to the professor of materia medica in the university, Dr. R. Stockman, in addition to the assistants already provided by the University Court. The bequest was formally accepted at a meeting of the Glasgow University Court on October 2nd. The other bequests of Mr. Weir include £10,000 each to the Glasgow Royal and the Glasgow Western Infirmary, £5,000 to the Victoria Infirmary, £2,000 each to the Kilmarnock and Ayr Infirmaries, and £500 each to the Glasgow Eye Infirmary and Glasgow Ophthalmic Institution.

The governors of St. Mungo's College have appointed Dr. John Henderson, one of the visiting physicians of the Royal Infirmary, to be Professor of Medicine in the college, in succession to Dr. T. K. Monro, recently appointed to the Chair of Medicine in the university. Professor Henderson takes up his duties on October 13th.

Medical Inspection of School Children in Glasgow.

Dr. Ernest Roberts, Chief Medical Officer of the Glasgow School Board, in his third annual report on the medical inspection of school children, states that of the total number of children examined during the school year 60 per cent. were found to be normal. In response to the request conveyed to them, 72.3 per cent. of parents attended at the medical inspection of their children. During the year 5,920 notifications of disease or defects were made to parents, and on re-examination of the abnormal children 33.4 per cent. were found to be cured and 28.6 per cent. improved. Twenty-six centres for the feeding of necessitous children were opened last session, at which 88,670 breakfasts and 419,872 dinners were supplied. The Board had appointed eight part-time dentists, three anaesthetists, three oculists, one aurist, one dermatologist, and six nurses to carry out the scheme of school clinics, and the medical staff employed in the work of examination included one male and two female full-time medical officers, fourteen part-time medical officers, and ten nurses. In the Board schools and other schools within the Board area no fewer than 45,728 pupils were examined for defective vision, of whom 26,430 were examined by the oculist, the remainder being tested by teachers.

Tuberculosis Treatment in Edinburgh.

On September 30th the Public Health Committee of the Edinburgh Town Council visited the City Hospital and inspected the extra accommodation provided for the treatment of phthisis under the Insurance Act. There has been room for some years in the hospital for patients suffering from advanced phthisis, but now additional accommodation has been prepared for those affected with the earlier stages of the disease, so that they shall not come into contact with the others. The new buildings are two large open-air shelters, capable of containing sixty patients each; the shelter is in the form of an elongated shed, with an entirely open front, and possessing a series of removable screens by which it can be partially closed in bad weather. At the end of each shelter is bathroom accommodation; each has also a large dining hall and a recreation room. The Edinburgh local authority is now in the position of being able to treat all types and stages of phthisis, and Dr. Maxwell Williamson reported to the Town Council's Committee that there was room for 180 patients in the City Hospital, that there were actually 88 patients in residence under treatment, and that there were therefore still 92 vacant beds available.

Declining Birth-rate in Edinburgh.

Some eight or ten years ago the medical profession pointed out the serious nature of the fall in the birth-rate in Edinburgh, but little public interest was excited thereby. During the past few years the rate has continued to fall, and the effect is now beginning to be seen in the attendance at the elementary schools. The local newspaper press is beginning to take some notice of the state of affairs. It is pointed out that, according to the School Board return, there is a decrease of 748 in the enrolments in the day schools for the session just opened as compared with last year. It is 36,960 as against 37,708, and the decrease affects 21 out of the 47 schools under the Board. Of course other causes, such as emigration, may be at work, but the falling birth-rate is a very important one. Thus, there are 57,170 houses in the city with a rental of £30 and under, and no fewer than 32,377 of these are without children of school age. It is claimed that with smaller classes the teaching will be more individualized, and possibly the hygiene of the schools will be better; but in the meantime the education-rate does not fall. It is perhaps by accident that one of the evening papers places, immediately after a note on Edinburgh's dwindling birth-rate, one on "taxation of bachelors," in which it is pointed out that "bachelors of both sexes" escape too lightly in the matter of taxes.

Causes of Infantile Deaths in Dundee.

Further evidence that there is need for the provision of rest for expectant mothers in the later weeks of their pregnancy and in the months following confinement is

found in the report of Dr. Charles Templeman, Medical Officer of Health for Dundee. Many of the married women in that city are at work all day in the mills and factories; this means that they are unable to take things easily before their babies are born, and it means that after confinement the babies are generally left in the charge of strangers, who are often old and incompetent, whilst the mothers return to their work. Dr. Templeman says:

> It is sometimes alleged that the disadvantages and dangers to infant life which attend the absence of the mother from her home during working hours are counterbalanced by the advantages which the increased earnings of the household bring to bear on the domestic comfort and well-being. From the point of view of infant mortality this, I think, is very doubtful.

The lady health visitors found that it was very difficult to convince mothers that milk was the only safe and proper food for infants; many women insisted on giving them a little of "what was going."

Extension of the Anderson Sanatorium at Hawick.

On October 2nd, at a meeting of the Hawick Parish Council, it was decided to proceed with the extension of the Anderson Sanatorium. It was stated that there was at present a lack of accommodation to deal with the cases of tuberculosis in which there was less hope of recovery. There was need for more outside shelters. Estimates had been obtained, and about £200 would be required from the Town Council and the same sum from the Parish Council. It was intimated that the Town Council had decided to provide its share, and Mr. Winning, who made the report, asked the Parish Council to do the same, and to this the meeting agreed.

Joint Sanatorium for North-Eastern Counties.

On October 3rd the Public Health Committee of the County Council of Aberdeen adopted the scheme which had been approved by the representatives of the County Councils of Aberdeen, Kincardine, Elgin, and Nairn, for a joint sanatorium. The scheme provides for 80 beds, and an administrative block sufficient to meet future extensions up to 120 beds. Under the approved arrangements 53 beds will fall to Aberdeenshire, 14 to Elgin, 10 to Kincardine, and 3 to Nairn.

Royal (Dick) Veterinary College, Edinburgh.

At the opening of the new session of the Royal (Dick) Veterinary College, Edinburgh, it was announced that the operations for the erection of the new college buildings are progressing rapidly, and would be pushed on with all speed in order that the classes might be transferred from the old buildings at an early date. The cost of the new buildings is estimated at £65,000.

Memorial to a Tiree Doctor.

On September 29th there was formally inaugurated at Baugh, Isle of Tiree, a memorial erected to the late Dr. Alexander Buchanan (Tiree). The memorial is in the form of a shaft of red granite 10 ft. high, resting on a solid granite pediment and encircled by a rough cairn of sea-worn boulders, the whole erection being 17 ft. high. The eastern face of the pediment bears the following inscription: "In loving memory of Dr. Alexander Buchanan, born at Callander, 4th October, 1835, died at Tiree, 29th April, 1911, for 51 years the medical officer and loved and valued friend of the islanders." The monument was unveiled in presence of a large gathering from all parts of the island and elsewhere.

Practical Course on Tuberculosis in Edinburgh.

As will be seen by an advertisement appearing at page 53, a practical course of instruction on the pathology, diagnosis, and prevention of tuberculosis (especially pulmonary tuberculosis) will commence in Edinburgh on October 29th. The course, which will last seven weeks, will be conducted by Sir R. W. Philip at the Royal Victoria Hospital and Tuberculosis Dispensary and by Dr. James Miller at the Pathological Department, Surgeons' Hall, and the Pathological Laboratory of the dispensary. The fee for the course is £7 7s. As the number attending the course will be limited, early application for places should be made to the Dean of the School of Medicine of the Royal Colleges, Bristo Place, Edinburgh.

Ireland.

[FROM OUR SPECIAL CORRESPONDENTS.]

The Dublin Hospitals and the Strikes.

The coal famine with which the Dublin hospitals were threatened owing to the present strikes in Dublin has been averted, and the various hospitals are now being supplied with coal under police escort. But a worse famine appears to be threateniag in the near future, and that is a famine in surgical dressings, and specially cotton-wool. It would seem from inquiries that have been made that the stock of cotton-wool in Dublin is only sufficient to last for about another fortnight, even with strict economy, and all the efforts made up to the present by the various medical supply agencies to obtain further quantities of cotton-wool from England have been unavailing, as it cannot be shipped to Ireland. It is said that an attempt even to bring it round to Dublin by the south of Ireland has failed. The supply of nitrous oxide is also reduced to such an extent that it will not last for more than about a fortnight. Unless the labour troubles come to a speedy termination, or some special means are adopted for the carriage of surgical dressings, a very serious condition will arise in Dublin, specially if there should be further riots, necessitating the treatment of surgical injuries in the hospitals to the extent of some seven hundred patients, as occurred in a period of less than three days about a fortnight ago.

Sanatorium for Tuberculous Joint Disease.

The objections of the residents in the neighbourhood to the erection of a sanatorium for the treatment of persons suffering from tuberculosis of the bones and joints, at Larch Hill, near Dublin, has led to the presentation of a petition signed by or on behalf of over 1,200 people living in the vicinity to the Dublin County Council and the South Dublin Rural District Council. The petitioners argue that the vicinity of the metropolis of Ireland is not a suitable place for the assembly of all these diseased persons, and that each county or province of Ireland should look after its own sick in sanatoriums or hospitals within its area. They are further of opinion that it is unwise and undesirable to place a hospital capable of containing some three or four hundred patients in a locality to which the citizens of Dublin are in the habit of resorting for fresh air and exercise. Here we think there must be exaggeration, as we do not imagine that there is any idea of founding such a large sanatorium; and anybody who knows Dublin and its neighbourhood must be surprised to hear it suggested that the place in question is a popular resort for fresh air and exercise. Finally a doubt is expressed in the petition as to where funds for the upkeep and maintenance of these numerous consumptive and other hospitals are to come from, the institutions mentioned being Crooksling and Peamount.

Local Government Board Report.

The forty-first annual report of the Irish Local Government Board furnished much interesting information. There has been a large decrease in the number of persons in receipt of Poor Law relief during the last five years; the total number at the end of last March was 24,861 fewer than at the same time in 1909, and nearly 3,000 fewer than in 1912.

Boarded-out Children.

The number of pauper children boarded out by the guardians was 2,589, in addition to 446 maintained in district and other schools. The total number left in the workhouses was 7,438, but 719 of these were in schools separate from the workhouses, which leaves a net total in the workhouses of 6,719. The number of orphans or deserted children, who alone are legally qualified for boarding out, was 1,506; of these 919 were unsuitable to be boarded out, owing to age, illness, physical or mental defects, or other causes. Therefore, the number eligible and suitable for boarding out was 647, or less than one-tenth of the total number. The guardians of eight unions did not adopt the system of boarding out; this accounted for 63 cases. In seven unions the guardians considered that the children were better treated in the workhouses than they would be if boarded out; this accounted for

another 90 children, and in nearly half the other cases the guardians complained that there was a difficulty in obtaining suitable foster parents or houses for the children. It would appear, therefore, that much more cannot be done in this direction, unless the guardians are empowered to board out all children in respect of whom they can legally assume parental control, as is the case in England.

Lunatics, Idiots, and Epileptics.

There was a decrease of 168, compared with 1912, in the number of lunatics, idiots, and epileptics in the workhouses, but there were still 2,145 in the workhouses in addition to those sick in hospital.

Causes of Death.

There has been a progressive increase in the number of deaths from cancer during the last four years; there was also an increase in the number of deaths from tuberculosis in workhouses, and this fact, taken in conjunction with the general decrease in mortality from this disease throughout the whole country, shows that these institutions are now used as places of isolation for advanced cases. The deaths of infants of under 1 year of age are still very numerous, the number for the year being 791. A woman doctor has been appointed as an assistant temporary medical inspector of workhouses, her duties being to make special inquiries as to infant mortality and nursing in the workhouses, and also to inspect children and the female wards. The staff of trained nurses has been increased to 268 in 146 homes, but there were still twelve workhouses where no trained nurse was employed; the number of pauper attendants had been further reduced during the year.

New Dietaries.

New dietaries framed in accordance with a list of rations prepared by the Local Government Board were tried experimentally in fifteen large and small workhouses in different parts of the country, and the result was highly satisfactory, the inmates being fed both better and cheaper.

Medical Service.

During the year there was an increase of 6,531 in the number of new cases attended at the patients' own homes, whereas the number of new cases attended at the dispensaries showed a decrease of 10,585. The boards of ninety-two unions have adopted scales of increased salaries for their medical officers, and in consequence the salaries of district medical officers during last year amounted to a total of £105,787, an increase of £1,591. The system of graded salaries was started in 1904, but the progress has been slow, and the Local Government Board expresses the hope that the boards of guardians which have not yet taken this subject into consideration will no longer delay in dealing with the claims of medical officers and granting them suitable increases in their salaries.

Midwifery.

There was a further increase in the number of dispensary midwives. The total is now 766, showing an increase of 210 in the last ten years, but there are still a considerable number of dispensary districts where the guardians have made no provision for supplying skilled midwifery nursing for the poor.

Vaccination.

In the previous report it was pointed out that there had been a marked diminution in the number of primary vaccinations performed by the Poor Law medical officers during the year ended March, 1912, as compared with the previous year, and this year there is a still further diminution. The decrease shown in this report in the two unions which include the city and suburbs of Dublin has been very considerable. The primary vaccinations carried out by the district medical officers of the North and South Dublin Unions in the year ended March, 1911, numbered 6,372; in the year ended March, 1912, 5,317; and during last year only 2,689. The local administration of the Vaccination Acts devolves upon the boards of guardians, and unfortunately some boards of guardians, including those of the two metropolitan unions, have adopted resolutions condemning the Compulsory Vaccination Act and refusing to prosecute defaulters. In some places the defaulters are not even asked to comply with the law. The Local Government Board is content to deplore this attitude on the part of the responsible local authorities and to warn them of the serious responsibilities in which they will become involved should an epidemic of small-pox occur in these districts. One case of small-pox was treated in a workhouse hospital during the year; it was the first since 1908.

Typhus Fever.

The number of cases of typhus fever notified was 126, but this, it is stated, does not indicate the true number of cases occurring during the year, as notification was not universal, and in the early stages of an outbreak many cases were not recognized and merely notified as fever. One doctor succumbed to the disease, contracted in the course of his duties while acting as temporary medical officer of Newcastle Dispensary District.

Notification of Tuberculosis.

The notification of tuberculosis had been adopted in forty-one sanitary districts, but even in these it was not thoroughly carried out, as shown by the fact that in both Dublin and Belfast the number of cases notified was less than the number of deaths from the disease. It is hoped that before long steps will be taken to introduce compulsory notification of tuberculosis in all the large urban districts; this is considered to be specially desirable in the county boroughs of Cork and Waterford, where the incidence of phthisis is particularly heavy. It was found that the Tuberculosis Prevention (Ireland) Act 1908, required to be amended in order to meet the altered circumstances consequent upon the passing of the Insurance Act. A short bill was accordingly drafted, and opportunity was taken to include provisions for the compulsory notification of all cases of pulmonary tuberculosis, but as these clauses were opposed they had to be deleted, as otherwise the bill would not have been passed. This is regretted, as it left Ireland in the unfortunate position of being the only portion of the United Kingdom where compulsory notification is not in force, although the disease is more prevalent in Ireland, and, consequently, notification is more urgently required.

Milk Supply.

In one hundred and eighty-five districts veterinary surgeons have been appointed as chief inspectors to superintend the work of the ordinary inspectors in carrying out the Dairies, Cowsheds, and Milkshops (Ireland) Order of 1908, and in one hundred and two districts other inspectors have been appointed; provision for the control of the sources of the public milk supply has thus been made in two hundred and eighty-seven out of a total of three hundred and nine sanitary districts.

England and Wales.

[*FROM OUR SPECIAL CORRESPONDENTS.*]

MANCHESTER.

SIR ALFRED HOPKINSON'S RETIREMENT.

THE retirement of Sir Alfred Hopkinson from the position of Vice-Chancellor of the Manchester University is now an accomplished fact, and the esteem with which he is regarded was never more plainly demonstrated than it was at a complimentary dinner given by the Convocation of the University on September 29th. Mr. C. T. Needham, M.P., Chairman of the Convocation, presided, and the company present numbered over 200. The Chancellor of the University, Lord Morley, was unfortunately unable to be present, and letters regretting absence were received from the Archbishop of York, the Bishops of Manchester and Salford, Sir A. W. Ward (Master of Peterhouse), Sir F. Forbes Adam, Sir Wm. Mather, and others. Sir Alfred was accompanied by Lady Hopkinson, and practically the whole of the teaching staff of the university was present. The toast of "The Vice-Chancellor" was proposed by the Chairman, who gave a brief sketch of the guest's career, and spoke in the highest terms of his work in furthering the interests of the Manchester University. Sir Alfred Hopkinson, when he rose to reply to the toast, was greeted in a most enthusiastic manner. He reminded the audience that he had occupied the position of Vice-

Chancellor for fifteen years, and only regretted that the daily routine of the office had prevented him from fully carrying out all that he wanted to do. He specially congratulated Convocation on its excellent choice of chancellors of the university. Three elections had been made, and he said deliberately that the three men chosen were men of whom English public life had reason to be proud. For their present Chancellor they felt not only veneration but affection. He spoke feelingly of the men with whom he had been associated in the earlier days of Owens College, men like Dr. Ward, Dr. Greenwood, Professor Jevons, Dr. Christie, and many others. As for himself, the proceedings of that evening would be an encouragement to him, and he should feel so long as he was able to work that those with whom he had been associated in Manchester recognized with a kindly heart anything he had been able to do. At the close of his remarks Sir Alfred Hopkinson was again greeted with cordial cheers. He intends shortly to go to India, and there can be no doubt that wherever he may be he will take with him the affectionate regard not only of Convocation, as the Chairman assured him, but of every member of the university and of the various public bodies which, largely through his influence, have worked so cordially with the university.

Scarlet Fever in Manchester.

Several weeks ago it became evident that an unusual epidemic of scarlet fever was commencing in the Cheetham district of Manchester, and in spite of the most energetic measures taken by the public health authorities the disease has spread to other districts. At the end of the week before last there were 430 cases in the Monsall Fever Hospital, and in a single week 150 fresh cases were notified. On October 4th there were 448 cases in the hospital, and 139 cases were reported in the week. As a general rule the cases have been of a moderate or mild type, and the fact that for several years previously the attack-rate had been very low in comparison with other large towns, has tended to cause more attention to be given to the present outbreak. It is noticeable that the most congested districts of the city, such as Hulme and Ancoats, have had only few cases; by far the greatest number have occurred in Cheetham, where the population is largely composed of Jews. In this respect the figures show the greatest contrast with those for 1912, when the attack-rate in Cheetham was one of the lowest, being less than one-third of the rate in the Harpurhey and Blackley districts. By far the largest number of attacks occurred in children between the ages of 2 and 5, though the case fatality was highest under one year of age. In 1912, taking the entire city, 77 per cent. of the cases were removed to hospital, and over 89 per cent. of those occurring in the Manchester township received hospital treatment, but an uneasy feeling has been expressed that the residence in hospital is not sufficiently long. This feeling probably arises from the fact that desquamation is not always completed when the patients are discharged from hospital, and the popular impression is still that the peeling is infectious. As a matter of fact, the alleged "return" cases were not numerous in 1912, and by far the most serious fact is that there are so many "overlooked" cases, meaning by this cases which were not recognized, and so not isolated, for at least a week after the onset. In 1912 no fewer than 103 of such "overlooked" primary cases were subsequently discovered. Of these, 26 occurred in homes where there were no "susceptible contacts," a "susceptible" being taken to be a person under 15 years of age who has not had scarlet fever, and no secondary cases occurred in these homes. The remaining 77 cases gave rise to 65 secondary cases in 47 homes. The total number of "susceptibles" in these homes was 190, and of these 65, or 34.7 per cent., contracted the disease. The fact that so many cases were not discovered for at least a week after onset, makes it practically certain that a large number of cases are never discovered at all, mostly owing to the mildness of the attack and the fact that no medical practitioner is summoned. In the present epidemic it has been impossible to admit all the cases to hospital; a careful selection is made, every care being taken to admit all cases occurring in homes where there are any susceptibles, or where sufficient isolation cannot be carried out. The question of closing the schools has been considered, but with the elaborate precautions that are being taken it seems to be felt that the children are really safer in school than running about without control in the streets.

LONDON.

Treatment of Tuberculosis.

A resolution passed by the Holborn Borough Council, and endorsed by the councils of several other metropolitan boroughs, with regard to arrangements for the treatment of tuberculosis in London are under consideration by committees of the Metropolitan Asylums Board. The borough councils express the opinion that the Metropolitan Asylums Board should be made the authority providing institutions for residential treatment of tuberculous persons of all classes, and that the borough councils should be represented upon the Asylums Board, so that the councils, as the local sanitary authorities of London, may be brought into closer relationship with the hospital authority.

The committees of the Asylums Board have also under consideration a letter addressed by the Local Government Board to the London County Council stating the opinion of the Local Government Board that the County Council should be the authority for the metropolis, and that through it all other agencies should be co-ordinated. The Local Government Board suggests that a great part of the accommodation required could be provided by the Metropolitan Asylums Board under agreement with the Council as regards insured persons under Section 39 of the Insurance Act, and as regards other persons under Section 3 of the Public Health (Prevention and Treatment of Disease) Act, 1913, and Section 80 of the Public Health (London) Act, 1891.

India.

[*FROM OUR SPECIAL CORRESPONDENTS.*]

Indian Women Medical Students.

The Principal of the Agra Medical School states in his annual report that 65 male students appeared at the final qualifying examination in April, 1913, and that only 29 passed in all subjects. On the other hand, of the 20 women students who presented themselves, 13 passed in all subjects. Of the failures among the male students 27 have been referred to their studies for a further period of one year, and 9 have been recommended for removal from the school, "as in the opinion of the examining board they are never likely to pass and their retention is a waste of money to the Government. . . . A large class of failed students also interferes with the work of other students and makes the classes unmanageably large."

The Indian Ambulance and Italian Explorers.

The Indian Branch of the St. John Ambulance Association offered the Italian Himalayan exploration party, on arrival in India, a complete sanitary equipment. The expedition was already largely provided with medical equipment, but Dr. de Filippi, the leader of the expedition, was so impressed by the utility of the surgical haversacks specially fitted by the association for his use that he gladly availed himself of them, and has written to Major Blackham, C.I.E., General Secretary of the Indian Branch, thanking him for the generosity and thoughtfulness of the Indian Branch. This interesting offer on the part of the Ambulance Association marks a new step in its career. Starting as merely an educational body, it has shown its ability at the Durbar and State Entry to organize large bands of trained men and women for public service, and now it has embarked on a new field of usefulness in designing and providing ambulance equipment for exploration parties. A body which has such a wide field of utility would certainly appear to deserve more financial support than it at present receives from the State and the Indian public generally.

Lunacy in the New Province of Bihar.

The Governments of Bengal and of Bihar and Orissa have agreed that the Berhampore Asylum shall continue, till other arrangements can be made, to receive insane

persons from those districts of the new province which formerly sent their lunatics to Berhampore. The Inspector-General of Civil Hospitals, Bengal, states that it has been practically decided that a Central Lunatic Asylum for Indians shall be constructed at Ranchi on a sufficiently large scale to meet the needs of the two provinces, and that it will be worked by the Bihar and Orissa Government, Bengal paying a contribution in proportion to the number of Bengal lunatics accommodated. Designs for the asylum are now under preparation by the Bihar and Orissa Government in consultation with the engineering experts and the Inspector-General of Civil Hospitals of both provinces and the Superintendent of the Berhampore Lunatic Asylum.

NEED OF SANITARY PROGRESS.

A painful illustration of the need for pushing on sanitary work in India occurs in the annual report of Nagpur City, which is by no means one of the worst cities in India from the sanitary standpoint. The total number of births registered in the city during the calendar year 1912 was 4,445 (40.69 per mille), against 4,440 (40.0 per mille) in the previous year. The total number of deaths during the year was 6,876 (63.4 per mille), against 5,506 (50.7 per mille) in the previous year. The high death-rate was due to plague, which continued in the beginning of 1912, and also to small-pox. The mortality amongst infants under 1 year of age also rose to 1,870 from 1,412, showing a ratio of 420.6 per 1,000 births in the year.

THE INDIAN BACTERIOLOGICAL DEPARTMENT.

The Government of India has under consideration the question of strengthening the Bacteriological Department. This is a very small department, consisting of only thirteen officers, who are mainly engaged in research work. It is understood that a scheme for augmentation of the staff has been drawn up.

Correspondence.

THE WORK OF THE ASSOCIATION.

SIR,—The proposition to increase the subscription to £2 2s. will, I trust, meet with general approval. I hope, however, that the change will not be made applicable to home members only, but will be universal throughout the world. I do not quite see why such a distinction as was suggested should be drawn. The expenses of all men vary. Some are rich, some poor. Income does not depend alone on the place of residence on the earth's surface, but upon many other factors.

The question really at issue is whether the work of the Association, which one may take to be (1) the spread of knowledge, (2) the preservation of the integrity of the profession, can be carried out on the present subscription. I am disposed to think it cannot.

The younger members of the profession do not, perhaps, entirely realize the very great importance of supporting an institution which may prove in emergencies their only practical and reliable safeguard against injustice, if not absolute ruin. Without it many would be helpless and powerless, as want of money, want of time, or the peculiar constitution of their work, may forbid the invocation of the law. It was, for instance, owing chiefly to the action of the Association that the Army Medical Service attained its present honourable and useful position. There are numerous similar instances. No one can foresee the blows that fate may have in store, and the wise man is he who endeavours to protect himself. There are few forms of insurance more reliable than the British Medical Association.

It is almost needless to refer to the immense amount of information the JOURNAL contains. To any one whose duty it is to know what is being done on the most advanced lines in every branch of work, the JOURNAL is invaluable. It is hard to see what the profession would do without it, especially those members who are not in immediate touch with great centres of work. Moreover it brings the reader into personal contact with the leaders of thought. Nothing could be more inspiring to a scientific thinker than the records of the ideas of the most advanced specialists of the modern world, so fully and ably reported in recent numbers.

But it is impossible to carry out work of the great scope of the Association without money; without funds, effective organization, either for purposes of defence and advance, or for the crystallization of knowledge, is impossible. It is to be hoped, then, that members will cordially approve the increase to the subscription, which could be borne by the profession generally, and further, that those who are members will induce those who are not, but who nevertheless reap many of the advantages of the Association, to join without further delay.—I am, etc.,

September 15th. SURGEON-GENERAL.

MODERN MEDICAL TERMINOLOGY.

SIR,—I have been meeting lately several general practitioners and antiquated fossils like myself who are breathlessly panting in the rear of modern medical progress, and trying to keep abreast of its recent developments, and from all sides I hear the same complaints.

First, of the complexity and obscurity of style and want of the "clarity" which one of your correspondents so earnestly recommended in your columns some time ago; and, secondly, of the harsh and jaw-breaking words, mostly from Greek roots, extracted with difficulty, tough to chew, and impossible to digest.

May I remind your readers of a few culled from recent numbers of the JOURNAL? "Trypanosomiasis" has, I believe, something to do with tropical diseases; and fortunately we are told that "triphenylmethane" is a dye, but no clue is given to the identity of "haptophoric." "Salvarsan" modestly cloaks itself in the mantle of "dioxydiaminoarsenobenzol," alongside, I suppose, of the "orthoamidophenol" group.

Pursuing our investigations a little further, we find "parasitotropism" and "organotropism," whilst a new derivative of quinine is introduced to us under the name of "ethylhydrocupinum," which is basely outdone by the methylhydrocuprein combined with pneumococcal serum. We are relieved to find that blastomycosis can be treated by salvarsan, but the victims of piroplasmosis must be in a bad way if they have to fall back on the two colouring matters—trypallavin and tryparosin. We hear of the semi-auricular spindle derived from the original semi-auricular ring, which has apparently something to do with the bundle system of the heart. It may possibly be satisfactory to some victims of cardiac trouble to be told that Külbs divided the bundle system into three distinct portions—namely, the Aschoff-Tawara node, the bundle of His proper, and the fine ramifications of the bundle to the apex of the ventricle, but even "orthopercussion" would fail to let me find the true inwardness of these weird terms.

If it is necessary to use a nomenclature which conveys no meaning to the ordinary professional mind then we must bow to the inevitable, and we old-fashioned folk must return with a sigh of relief to the clear and classic pages of Watson and Clifford Allbutt and Paget and Latham and Chambers.—I am, etc.,

Finzean, Aboyne, Sept. 26th. R. FARQUHARSON, P.C., M.D.

AFTER OUR HOLIDAYS.

SIR,—The end of all pleasant things is tinged with sadness, and the end of our holidays is usually a little annual tragedy. And the disaster is greater if it is not. For men and women who love the crowded streets and long to get back to them, are abnormal beings; they have shed their healthy instincts. I am always suspicious of the person who exclaims, "The country is too slow for me!" There was a man who was recommended to go for a month to Tunbridge Wells, but came back to London after a stay of a day, saying, "I couldn't stop there. It's all trees!" Persons of this sort may not be sorry when their holiday ends; they have got so used to an artificial life that a natural mode of existence bores them. But they have soon or late to pay a heavy price for their perversity.

Even those of us who looked forward with zest to an escape from the smoke and whirl of the city will often feel at first disappointed with the effect of the rest. We get so used to wearing harness that we feel uncomfortable without it. This feeling, however, rapidly passes off as a rule, and something lifts from our minds as soon as we get clear from the great city and catch glimpses of the quiet villages of the green countryside, with a fresher, cleaner

air blowing across it. But it still requires a little time before we cast off the irritating and wearying habits of the town. We miss the hum of the streets and the frequent calls of the postman; and so strong has become the newspaper habit that those of us who bury ourselves in the country continue for some days to long for our morning and evening sheet of news. Yet slowly and surely the peace of the fields descends upon us, and we find ourselves content to drift instead of drive through life. The days glide away and carry us with them, and we leave all thought of time and work behind us, and our minds gradually become engaged more with the sun and moon and stars than with the transient things of the earth. This is the general effect. For you may walk up a village street and find that not one-half of the people know the day of the month, and that some are even uncertain about the day of the week.

That is the right mood for a holiday. To sit on a lawn, or hill, or cliff, and do nothing of one's own will, and let the restoring effects of nature play on us is the height of wisdom. How kindly does our mother and our nurse give us our sedative! All the fever and the fret leave us, and we gather new strength with as little effort as we draw our breath. These are among the highest and the fullest moments of our life—the moments when the best part of us comes uppermost, and we feel most grateful for our creation and our preservation.

The great mistake of many holiday makers is to go to a crowded seaside resort. They apparently believe that they can improve their health by living in a hot lodging-house, eating indifferent food, and taking no exercise, while sleeping, smoking, and often drinking a great deal more than is necessary. They would be better off in body and pocket if they kept to their work. I have been pained at times to find persons returning from their holidays with a tired body and a weary mind. The change should have been taken quietly. No good can come out of a pleasure trip spent in travelling by rail or boat at a speed of five or six hundred miles a week, while trying to see the sights of the cities and the beauties of the scenery by the way. In this respect our American cousins are greater sinners than we. Arriving at a town, they will snatch a hurried meal and rush out to do the sights in time to catch the next express. Their strained, nervous faces show what good they derive from this extraordinary way of enjoying oneself.

The holiday which does the holiday maker most good is a holiday of leisure and early hours, as well as of change of scene and new interests. And those who relish keenly a spell of country life spent in this manner always feel sad at having to leave it for another year of toil in the city. Yet we must all admit the truth of Shakespeare's remark:

> If all the year were playing holidays,
> To sport would be as tedious as to work.

We find proof of this in the lives of those who are idle and rich. Killing time becomes in the end a tedious task. What we wish for, if we are wise, is just the leisure to follow some hobby. Well, if we cultivate that hobby as best we can, life will soon have more flavour.

To live in a big city, to catch the same train, to go to the same office, sit on the same stool and do the same work, year in and year out, does take the heart out of a worker. It is the dull monotony of it that breaks down a man's nervous system and produces melancholia, irritability, and hopelessness. Many men and women come back from their fortnight's holiday with a breaking heart, knowing they must tread the same weary way in the vast human hive. Yet the hive goes on increasing in size and population. Great cities are, in my opinion, great curses; they are vices, and, like all vices, fascinating. Still they grow, and still the flight of the country folk continues—still the stir and lights and lure of higher wage draw into the destroying cities those whose lives have been simple. The remedy we do not know. Of one thing, however, we may be quite certain, and that is that the health of the individual is always damaged by a town life.

Indeed, I am inclined to think that if a high wall were built round London and the inhabitants confined therein and well fed and housed, in a few years—probably little more than twenty-five—there would not be a soul alive. We cannot even under present circumstances find more than four generations of Londoners. Life becomes more complex every day, and although we have done much to accelerate speed and diminish distance, we cannot claim that these advantages are to the increased health of the race. Those who have returned to London after a quiet summer holiday must find the noise and the atmosphere worse than before. It takes the refreshed worker some time to settle back into his groove; the harness chafes at first—sometimes it always chafes—because it does not fit. All we can do is to remember that happiness can only come to each of us in patches, and to take care of every fragment when we find it. Half of the work any human being does is distasteful, and the two qualities that will help us most are cheerfulness and courage.—I am, etc.,

October 6th. M.D.

THE ORIGIN OF LIFE.

Sir,—In the course of the recent discussion on the above subject following Dr. Bastian's paper at the International Medical Congress, an important suggestion has been made by Dr. Bothwell Thomson—due, I believe, originally to Mr. George Barlow—to the effect that the primitive germs to which life on our planet is due, came originally in the form of a corpuscular radiation—such as light is now supposed by many physicists to consist of—and that this radiation came to us from the sun. It is not, of course, supposed that these corpuscles resembled the living organisms of to-day, nor, indeed, those contemplated in Lord Kelvin's and Arrhenius's hypotheses, since the heat of the sun would have rendered their existence impossible, and, had they come to us from interstellar space, the light from the sun would have been sufficient to destroy them, so far as their potential vitality was concerned, at a distance far beyond our own from our luminary. But if, as I understand, the suggestion is that the corpuscles are electrons themselves, the theory is quite a plausible one. In an article on "Radio-activity" in the *Monthly Review*, November, 1903, I pointed out that the corpuscles from radio-active substances might possess such properties. These corpuscles are the same as those from the sun and nothing more or less than electrons. The idea was subsequently developed in my contributions to *Nature*, May 25th, 1905, and the *Fortnightly Review*, September, 1905, and more fully in my book on *The Origin of Life* (1906).

The cellular growths produced by radium are due doubtless to the emanation, which has the property of coagulating albuminoids, so that artificial cells appear to multiply as the result. Moreover, if any traces of sulphur be present the radium will cause a sulphate precipitate, and the radium, in virtue of its physical properties—unlike barium, strontium, or lead—will act on the sulphate as Sir William Ramsay has just found, converting it into argon, thereby adding to the growths.

These "mimic cells" are quite different from Buschli's, Lehmann's, or Leduc's, though they appear to have been observed by Dubois while the experiments were being made at the Cavendish Laboratory at Cambridge, and subsequently at the pathological laboratories. They are, of course, not necessarily alive, or to be identified with the living organisms of the evolutionary series; but, as M. le Dantec of Paris says, it is the opinion of the learned world that life might be sought in the radium atom, or, at any rate, that great enigma to the physicist—the positive ion or nucleus of the atom. These bodies, which I have named "radiobes," are so unstable that the light from the sun, presumably its heat, is sufficient to melt them.

It is true that radium itself is a poisonous substance like strychnine, although in very minute quantities it may act as a stimulant to living matter. Light acts in this manner.

It would be of interest to know whether Dr. Bastian has succeeded in stimulating the growth of his cultures by exposing them to radio-active radiation instead of to diffused daylight. But it must not be expected that any bodies which might arise spontaneously at the present day would be actually bacteria, but rather such as could merely be placed in the borderland between living and dead matter in the evolutionary series which has been handed down to us through countless generations and innumerable ages in the past. The most elementary forms of life in nature known to us to-day are the descendants of

an almost infinite number of ancestors, whereas those of the laboratory can be at the most but of a few.—I am, etc.,

Royal Societies Club, St. James's Street, W., Oct. 4th. JOHN BUTLER BURKE.

FIBROSITIS AND MUSCULAR RHEUMATISM.

SIR,—I have been very interested by the discussion on fibrositis and muscular rheumatism which is reported in the JOURNAL of October 4th.

My interest, I must confess, is mainly on personal grounds, as I have been through one very severe attack of muscular rheumatism eight years ago, and have had it more or less constantly with me ever since.

Dr. Luff's account of the disease is by far the most comprehensive that I have come across, in a fairly exhaustive search of the literature of the subject, and I think he has earned the gratitude of the profession for putting the case so clearly and fully before us.

The articles in most current textbooks on muscular rheumatism are so inadequate, and in many cases so inaccurate, that it is wellnigh impossible to get a clear idea of what the disease means or how to treat it.

My own case was rather interesting. After lying for about half an hour on a sunny day in May on a grassy cliff, I was seized that night with pains nearly all over my body, but especially in the abdominal and lumbar muscles. I had no rise of temperature at any time, and after three weeks' treatment in bed with salicylates, iodides, and alkalis, and most of the usual antirheumatic drugs, I was no better. I was then sent to Harrogate, and my medical adviser there, after demonstrating "nodules" to me in my lumbar muscles and abdominal recti, ordered me a course of Harrogate massage, douches, and sulphur water to drink.

At this time I was only able to walk for a very short distance on crutches, and could not lie down in bed, but had to be well propped up to ease the strain on the recti. Most movements were very painful, but especially any attempt at laughing, which seemed to tear me to pieces in the neighbourhood of the pubis. The massage was at first intensely painful, and I groaned so freely that my bath-man wanted to stop after a few minutes. However, I persevered, and my cure was rapidly accomplished. I soon got rid of the crutches, and from having the appearance of an old man of 75 I returned to my normal gait as a youth of 25. In three weeks I left Harrogate, and was able to take up my house-surgeon work again with full vigor.

The most troublesome part was at the insertion of the recti at the pubis, and it was some weeks before I could do away with all except one pillow under my head at night. Since then I have had several minor attacks, and on one occasion I had to resort to massage again, but not being within reach of Harrogate I had to train an attendant at an ordinary Turkish baths how to apply the necessary movements.

I find that sodium salicylate has no effect on me beyond making me miserable, but aspirin really does give some relief. Diet does not seem to have much effect one way or another, although I nearly always tell my patients to keep to milk foods. I find that beer is absolute poison, as is also port.

I have never had the opportunity of trying the various kinds of electrical treatment, but sea bathing I really do find to be of certain use. I may be feeling as stiff as possible after a wet spring and early summer, but with my first swim my pains go from me like magic, and I keep the bathing up as long as possible into the autumn. Even now, although the weather has thoroughly broken up, I am going in for another few dips to help me to start the winter well. I was surprised to see that none of those who took part in the discussion suggested this, and if, perchance, they have not thought of it I bring it to their notice. I confidently recommend it to all my patients, and they get great benefit from it.

I suggest that muscular rheumatism is really a perineuritis, as one finds not alone that the painful points coincide with the points of exit of nerves through the muscles and tendons, but that the "nodules" can be felt at these sites.—I am, etc.,

Brixham, South Devon, Oct. 6th. CECIL B. F. TIVY.

ON DOSES IN THE NEW "PHARMACOPOEIA."

SIR,—Very little has transpired as to the changes that will appear in the new *Pharmacopoeia*, but it is to be hoped that the present chaotic table of doses will be simplified. Sir William Osler has recently complained of the immense amount of knowledge exacted from students, and here is one way of reducing it. But this is only a single aspect of the subject. Probably every reader of this paper will be able to recall an occasion when the patient was deprived of the benefit of the most promising drug because the prescriber was not quite certain of the dose, something else not so good being substituted. This would happen chiefly at visits, though even in the consulting-room it does not do for the doctor to be seen referring to a book for the dose. But, worse even than this, it is quite certain that there have been at least a few deaths from the same cause, and many more have been prevented only by the vigilance of the chemist. The subject is specially urgent because under the Insurance Act numbers of men who have almost forgotten the art are now called upon to write prescriptions.

Two years ago, in a little book on *Treatment*, I wrote these words: "It is to be hoped that in the next *Pharmacopoeia* all galenical preparations of the same name will have the same dose," and I made the suggestion that strong preparations should have the prefix "per." I propose now to go into the matter somewhat more fully, and I hope to show that the golden maxim, "one name one dose," can be adopted without disturbance to old practitioners.

Some preparations already conform to the principle. Such are the confections, the decoctions, the mixtures, and practically the syrups, while to aquae there is only one exception. On the other hand, the sixteen liquid extracts have 6 different doses, the twenty-two infusions 3, the forty liquors 11, the fifteen spirits 4, the five succi 3, the sixty-seven tinctures 3, and the six vina 3. Is any living person capable of remembering every one of these doses in addition to those of the simple drugs? I doubt it. There is, it is true, a rough guiding principle which teaches us that the dose of a tincture is usually ½ drachm to 1 drachm, of an infusion ½ oz. to 1 oz., and so on; but when, as here, exceptions are numerous, such a rule is worse than none. It becomes, indeed, a most dangerous snare.

But if we are to adopt the suggestion of having only one dose for every preparation of the same name, it is evident that there are not enough names to go round, and we must reconcile ourselves to new ones. I propose, therefore, to retain the present name for the principal preparation and to prefix "per" to those with a smaller dose, and "sub" to those with a larger one. There would still be divergencies, but these could be reduced by standardization to a single dose for each class, the dose being increased by dilution or diminished either by evaporation or by using less of the menstruum. With such horrible words in current use as uricacidaemia and antibody, I need hardly defend my Latin. Per, however, is of course used as an intensive, and my compound words have their classical counterparts in *perturbatio* and *pernegatio*. Pertincture, therefore, means a strong tincture, and subtincture a weak one. The most objectionable word is perinfusum, but perfusum would mean poured through; after all, infusum has been turned from a participle into a noun, and we have an analogy to perinfusum in perfectus as a noun, and in perincertus as an adjective.

It is difficult to bring the liquid extracts into the scheme, but it can be done by omitting those of nux vomica and ipecacuanha, which are practically used only for making a tincture and a vinum. With liquors, in their present ridiculous arrangement, I must confess myself baffled, their eleven different doses ranging from ½ minim to 2 oz., while thirteen have no dose at all! Some changes, however, are certain to be made, and these must bring them into line.

My idea, therefore, in its completeness is to make the dose of every tincture ½ drachm to 1 drachm, pertincture 5 minims to 15 minims, and subtincture ½ drachm to 4 drachms; every infusion ½ oz. to 1 oz., perinfusion 2 drachms to 4 drachms, subinfusion 1 to 2 oz.; every spirit 20 to 40 minims, perspirit 5 minims to 20 minims, subspirit 1 drachm to 2 drachms; every succus 1 drachm to 2 drachms, persuccus 5 minims to 15 minims: every

liquid extract ½ drachm to 2 drachms, perextract 5 minims to 15 minims, subextract 2 drachms to 4 drachms; every vinum 1 drachm to 4 drachms, pervinum 10 minims to 30 minims, and subvinum ½ oz. to 1 oz.

Of course objections will be raised, and I will consider some in advance. There would be absolutely no danger in the change, because no preparation would be stronger than that of the same name in the present *British Pharmacopoeia*, while with drugs like ipecacuanha, to which two doses are accorded, there would be no more and no less difficulty than at present. Old practitioners, therefore, could mix up the old and the new titles with impunity.

The one thing with which the maxim, "one name one dose," would clash is the table of strengths. Now, a knowledge of the strength of a tincture or solution may or may not be of use to the pharmacist, but to the prescriber it is only of value as an indirect guide to the proper dose, and the system here advocated makes it so easy to remember doses that an indirect guide is superfluous. I may add that I have examined the pharmacopoeias of France, Germany, Austria, the United States, and Japan (Latin edition), but in all these is the same want of system as in our own. Let us give them a lead!—I am, etc.,

London, S.W., Oct. 4th. RALPH W. LEFTWICH, M.D.

TURBINECTOMY.

SIR,—It is difficult to understand why Sir Robert Woods condemned this simple operation in such sweeping terms (reported in your issue of September 20th), and I am glad that Dr. William Wilson has drawn attention to his remarks. Many years ago I thought, as Sir Robert Woods does still, that a dry nose and throat were the inevitable sequelae of all turbinectomies, but experience has proved to me that the textbooks are wrong in their statements on this point. When the inferior turbinates are large and thickened with chronic inflammation and cause obstruction to nasal respiration I am convinced that a removal of a portion of the thickened turbinate is not only a justifiable operation, but one which gives excellent results, and all the patients who have undergone it express themselves as very grateful for the relief which they obtain after this simple and painless operation. Only those who have suffered from enlarged turbinals realize the discomfort which they cause, and I have cured many headaches of years' standing by removing one or both inferior turbinates. The risk is practically *nil* and the pain very slight. I have only had 3 cases of troublesome haemorrhage in 147 cases; those were all young females, and in them intranasal plugging was necessary for twenty-four hours. I never use a spokeshave, as I consider it an unscientific instrument. I make a preliminary cut with a special pair of scissors and finish with a cold wire snare. It is important, as I pointed out ten years ago, not to mix adrenalin chloride with the cocaine solution, as it occasionally leads to smart haemorrhage about one and a half to two hours after the operation.

In some cases of deflected septum I prefer turbinectomy to a submucous resection of the septum: this will no doubt sound like rank heresy to many rhinologists, but the results have justified my method of procedure.—I am, etc.,

Lincoln, Oct. 6th. STANLEY GREEN.

SIR,—As to the merits or demerits of this operation, there seems to be a growing opinion condemning its general application. The turbinal mucosa, playing as it does such an important part in the nasal respiratory functions, ought to be at all times, as far as possible, respected. An adequate breathing channel can usually be obtained by submucous resection of the bony portion of the turbinated bodies. Mutilation of physiological tissue is thus spared, and such sequelae as pharyngitis sicca and rhinitis sicca are avoided. This method of reducing the turbinated bodies requires special skill and practice, but fully repays the t[illegible] involved. No doubt many partial turbinectomies a[illegible] done where the septum (which in all cases should receive the first attention) is the offending member. Turbinectomy, partial or complete, might with advantage be abandoned, save, of course, as a preliminary step in sinus intervention, in exceptionally narrow noses where no other means are available, and perhaps in very obdurate cases of paroxysmal rhinitis.—I am, etc.,

London, W., Oct. 4th. H. CLAYTON FOX, F.R.C.S.Irel.

SIR,—I think Dr. Wilson has misunderstood me. Against what he calls a "carefully graduated turbinectomy" I feel sure I should have nothing to say, except that his terms appear contradictory. The word "turbinectomy" should be reserved for complete removal of the (inferior) turbinal, both soft parts and bone—that is, the operation for which the spokeshave was devised. It is only against this operation that I raised my voice.

Every one agrees that "an advanced condition of chronic hypertrophy" should be treated by removal of a portion of the turbinals (turbinotomy), but that is another thing, unless it is carried to excess. In the third paragraph of his letter Dr. Wilson misquotes me. I did not say that "atrophy was a necessary consequence of this operation and not a complication." What I did say was that pharyngitis sicca often occurs after removal of the turbinals; and by removal I meant complete removal, or turbinectomy. My remarks about instruments had reference to the only instrument adapted to that purpose—namely, the spokeshave.

Though I do not think my words were ambiguous, I might, perhaps, have been more explicit had the time for my paper not been limited to fifteen minutes by the clock.

With Dr. Wilson's remarks about the rudimentary state of our knowledge of the physiology of the turbinals I quite agree.—I am, etc.,

Dublin, Oct. 7th. ROBERT H. WOODS.

CURE BY LAUGHTER.

SIR,—Your note in last week's JOURNAL (p. 879) on the curative effect of laughter recalls to my mind a personal experience. When I was about 23 years of age I had a very severe attack of quinsy; swallowing was almost impossible, and breathing difficult. The surgeon in attendance was about to incise the tonsil, but while waiting for a scalpel he took up the *Ingoldsby Legends*, which happened to be lying on the table. He proceeded to read the diverting history of "The Lady Rohesia," and how she was cured of her quinsy. In spite of the miserable condition in which I was, the story caused me to laugh, and this led to the bursting of the abscess, and to my cure without the use of cold steel.—I am, etc.,

London, W., Oct. 6th. F. DE HAVILLAND HALL.

VOICE AND SPEECH TRAINING.

SIR,—The BRITISH MEDICAL JOURNAL of August 30th, 1913, contains on p. 558 the report of Subcommittee No. 1 of the Voice Training Conference of the University of London on "normal healthy breathing in relation to speech." On my return from abroad my attention has been drawn to statements therein made, which in some respects are so extraordinary that they can hardly be allowed to pass as authoritative.

It will be remembered that some few years ago one of the signatories, Professor Arthur Keith, took an extremely keen interest in the mechanism of respiration, and since I was then also actively engaged in pursuing researches on respiratory phenomena, I had full opportunity of becoming personally acquainted both privately and publicly with the conclusions which at that time Dr. Keith had reached.

Portions of the report of the Subcommittee above referred to are, of course, truisms which no one is likely to controvert, but in justice to Professor Keith's previously published work, I would ask him whether the report of this Subcommittee represents his present considered opinions.—I am, etc.,

London, W., Oct. 7th. J. F. HALLS DALLY.

THE MEDICAL ASPECTS OF THE CENSUS PAPER.

SIR,—In the suggestive note on the medical aspects of the next census paper (September 27th, p. 824) members of our Association are invited to make proposals as to questions the inclusion of which in the next census

inquiries may be productive of useful information. This has for years been a hobby of mine, and I have thought much about it. The census might be made enormously more useful than it ever has been, and information might be obtained which would help in the solution of some medical problems. For instance, the decay of the teeth so enormously prevalent. Has this anything to do with locality and water supply? I have a strong impression that it has. Now, wages are much better in Lancashire, and the people are better fed than Londoners, and yet during the last fifty years, during which time I have almost yearly visited London, I have always been struck by the apparent fact that the Londoners seem to have much better teeth than the Lancashire people. In Lancashire it is quite common to meet young persons only just out of their teens with not a tooth of their own growth in their mouths. Healthy, well-developed, young people, too, not the least delicate, and coming of healthy stocks. If this is so, what is the explanation? I have a hypothesis that the reason is that the London water contains plenty of lime and the soft drinking waters of Lancashire almost none. The census, and only the census, could settle this.—I am, etc.,

Blackpool, Sept. 30th. WILLIAM HARDMAN.

Medico-Legal.

ACTION AGAINST A PANEL MEDICAL OFFICER.

IN Liverpool County Court on October 2nd his Honour Judge Roberts heard an action brought by an insured person against a medical man on the Liverpool panel. Judgement was given for the sum claimed, but, in view of a possible appeal on a point of law, proceedings were allowed to be stayed for twenty-one days.

The plaintiff's claim stated that he was an insured person under the Insurance Act and entitled to its benefits; that the defendant was a panel doctor who in January accepted the plaintiff as a patient whom he would attend; that at the end of March the plaintiff became ill and called upon the defendant for his services, and these the defendant negligently and improperly failed or declined to supply. Hence the plaintiff had to engage another medical man whose charges amounted to £2 6s., which sum the plaintiff consequently claimed from the defendant.

The defendant's reply notified that he intended to rely on the second section of the regulations issued by the Insurance Commissioners, and on Rule 1 of the rules adopted by the Liverpool Insurance Committee in February for the administration of medical benefit.

The evidence for the plaintiff as reported in the *Liverpool Post and Mercury* was to the effect that after playing football on Saturday, March 29th, he contracted a chill, and the following morning began to spit blood. A messenger who was sent to ask the defendant to attend did not find him at home, but wrote down the name and address of the patient, and left word that the case was urgent. A second messenger at 3.15 p.m. found the defendant at home, and was informed by him that the earlier summons had come too late for him to call on his morning round. On a request being made that he would now come at once because the boy's mother thought he was suffering from pleurisy, the defendant, it was alleged, replied that he was not supposed to see patients on Sunday, and if necessary would report the matter to the Insurance Committee as a vexatious case. At 6.25 p.m. a third message was delivered by two married sisters of the plaintiff, who, on asking whether the defendant intended to visit the patient, received, it was alleged, the reply, "I can't come now. . . . I want recreation just as much as any one else does. I will come as soon as it suits me." In the event the defendant did not attend at all, and about 8 p.m. the boy's condition became such that another doctor was called in and took up the case, which he found to be one of pneumonia.

On opening the case for the defence counsel submitted that the plaintiff had no right of action against the defendant. The Act provided a remedy for his grievance, and that remedy was the only one of which he could avail himself. Provision was made for every dispute by Subsection 2 of Section 67 of the Act. It was the duty of the Insurance Committee to provide insured persons with medical treatment, and in pursuance of that duty the Insurance Committee made arrangements with the doctors. Hence it was with the Insurance Committee alone that the plaintiff could deal. If there had been any default on the part of the defendant—and this counsel did not admit—then it was the Committee's default, and the plaintiff must call the Committee to answer for it. Alternatively counsel pleaded that if the foregoing view were not accepted, then the matter should be regarded as a dispute between a doctor and an insured person, and under Subsection 52 of the Regulations drawn up by the Commissioners such disputes should be dealt with by a special subcommittee appointed by the Local Insurance Committee.

In reply to this argument, Judge Roberts suggested that if the first plea were well founded it meant that an Insurance Committee was itself to act as the judge in respect of any grievance alleged against it. Counsel responded that this was not the case, because it was the Committee's duty to set up a special subcommittee to deal with such allegations. The court then further suggested that the section of the Regulations quoted by counsel was merely intended to apply to questions arising as to the conduct of a doctor when actually attending a patient, and not to cases in which a practitioner failed to attend. Was not the reference to the Insurance Committee for the purpose of discussing whether the doctor should be retained on the panel, or reprimanded, or dealt with in some disciplinary way; or, on the other hand, whether the insured person in any given case had committed some offence, such as treating the doctor vexatiously? Finally, the court decided the case must proceed.

Counsel for the defence then continued his opening statement. The defendant, who had only about 450 insured patients, made no distinction between them and his private patients; his hours for seeing them were the same and all went into the same surgery. The plaintiff's friends must have known by 6 a.m. on Sunday that he was seriously ill and ought to have given notice promptly to the defendant, who it so happened actually visited during his morning round the street in which the plaintiff lived. When he returned from his round about 3 p.m. he was late for his dinner and had to attend in his surgery from 3 p.m. to 4 p.m. He accordingly had a snack of dinner and got into his surgery about 3.10 p.m., remaining there until 4.50. At 5 p.m. he took a cup of tea, and at 5.10 a second messenger arrived. He told this messenger that he would come to the house, and then returned to finish his tea. At 5.30 p.m. just as he was about to start to see the plaintiff, the two sisters of the latter arrived and insisted that the defendant should go with them at once. He was nettled by their manner, and because he did not at once comply with their demand they said he need not come at all and that they would get another doctor. This statement the defendant took as his dismissal from the case and he considered it no further. The plaintiff's parents, counsel submitted, had acted unreasonably in not giving the defendant more prompt notice of the need of his services. In regard to the statement that the defendant had been apprised that the need was urgent, all calls were urgent from a patient's point of view, and it was a medical man's duty, if he were to get through his work at all, to discriminate what cases really justified this term.

Evidence in support of his counsel's opening address having been given by the defendant, the latter under cross-examination stated that if the boy was spitting blood at 6 o'clock in the morning the disease from which he was suffering was well established and he ought to have been summoned much earlier. He never refused to go to the case: he only refused to go at once. He did not realize that it was an immediate case; an immediate case wanted immediate attention, but he drew a distinction between an immediate case and an urgent case. In medical phraseology "urgency" did not always mean that the case should be attended immediately. At the time the two sisters called he certainly was not occupied, but he had just finished his tea, and had been very busy up to that time. He then realized that it was an urgent case, but refused to go at once because he was asked to do so in an offensive manner. This offensive manner was not his only reason. He was going to stay at home and have a rest and smoke for five or ten minutes. He had not declined to go at all, but only to go there and then. It was not true that he had threatened to bring the matter before the Commissioners if it proved to be a vexatious case. In reply to a suggestion of the court that, as he must have seen that the plaintiff's sisters were extremely anxious and alarmed, he might have said "I am coming," the defendant repeated that the reason he refused to go was the extremely offensive way in which they ordered him to do so. Doctors did not like to be ordered about.

On conclusion of the final arguments on either side, His Honour delivered judgement to the following effect: The case raised for the first time, he believed, what might prove to be a difficult question of law—namely, what were the legal relations between a person under the Insurance Act and a medical man who had accepted him as one of his panel patients? He did not agree that if a panel doctor failed to carry out his duties the only remedy open to an aggrieved panel patient was that given under certain regulations in a certain section of the Insurance Act. The regulation to which counsel had referred seemed really to be a disciplinary regulation and was intended to govern cases which arose during the course of the treatment of a patient. There was nothing in it which excluded a patient from proceeding against a doctor who in the course of his treatment had acted negligently provided that such remedy would have been open to the patient in ordinary circumstances. Once a medical man had accepted the duty of attending a patient he was bound to do so with due diligence, whether the patient was an insured person or otherwise. In the present case, as a matter of fact, the doctor did not attend the patient at all, and the question arose whether, when a doctor had wrongfully not given his services, and when thereupon the patient reasonably called in another medical man, the doctor was not liable for the expense involved. Under the Insurance Act the patient paid a contribution which entitled him to the services of a panel doctor and to freedom from the obligation of having to pay fees to an ordinary doctor. If the panel doctor who had accepted the patient wrongfully refused to attend him, could a patient himself sue the doctor directly for any expenses to which he might properly be put by calling in another doctor?

This question, the judge thought, should be answered in the affirmative. A panel patient could not get the free services of another doctor without getting a transfer, and to do this must take time, which was not available when cases requiring immediate attention were in question. In the present action there were some discrepancies in the evidence as to the hours at which messages were delivered and other matters, but when the third message was received the defendant ought to have realized that the case might turn out to be really urgent, and that he had no business to say "I will come some time." The probable explanation of his refusal to do so was not inhumanity or anything of that kind, but merely that he felt that he ought to have been sent for earlier, and was vexed at the way in which he was being pestered. His answer arose from the temper of a busy man on a busy day, and in the circumstances the plaintiff's family was justified in calling in another doctor and entitled to charge the latter's bill against the defendant.

His Honour then entered judgement for the plaintiff with costs on scale B, but on application from the defendant's counsel granted leave to appeal.

The Services.

ROYAL NAVAL MEDICAL SERVICE.

EXAMINATION FOR APPOINTMENTS AS ACTING SURGEONS.

AT the examination for the Naval Medical Service held on September 29th and October 1st and 2nd, thirteen candidates were successful, and obtained the following marks:

Name	Marks.	Medical School.
Mr. J. S. Elliot, M.B. ...	1,703	... Edinburgh University.
Mr. A. J. Tonkinson, M.B.	1,686	... London Hospital.
Mr. A. R. Sharrod, M.B.	1,643	... London Hospital.
Mr. T. J. KilBride, M.B.	1,578	... Cecilia Street Medical School, Dublin.
Mr. A. E. Panter, B.A....	1,515	... Cambridge University and King's College Hospital, London.
Mr. G. S. Harvey, M.B...	1,483	... University College, Cork.
Mr. J. P. Blockley, M.B.	1,470	... Edinburgh University.
Mr. W. H. Murray ...	1,455	... University College, Cork.
Mr. J. R. Haldane, M.B.	1,455	... Glasgow University.
Mr. G. H. Hayes, M.B....	1,435	... University College, Cork.
Mr. W. S. Birch	1,418	... King's College Hospital.
Mr. H. C. A. T. Cannon	1,380	... Royal College of Surgeons, Ireland.
Mr. H. J. Hopps, M.B....	1,338	... Edinburgh University.

The maximum number of marks obtainable is 2,4

Universities and Colleges.

UNIVERSITY OF CAMBRIDGE.

THE following candidates have been approved at the examination indicated:

SECOND M.B.; *Part II (New Regulations)*.—A. C. Ainsley, A. B. Appleton, K. Atkin, G. A. Back, B. F. Bailey, O. A. Beaumont, P. H. Berry, W. K. Bigger, C. E. Bond, A. B. Bratton, H. C. H. Bull, B. Burnside, F. V. Cant, F. L. Cassidi, G. F. Clifton, L. P. Costobadie, H. E. Cresswell, J. S. Eloff, E. T. D. Fletcher, L. S. Gathergood, A. A. Gemmell, G. L. Grant, C. Habgood, H. W. Hales, W. F. T. Haultain, R. C. Hewitt, E. P. Hicks, W. Hillbrook, C. E. Kindersley, W. N. Leak, A. A. Lees, B. K. Liang, H. D. McCall, J. B. McFarland, H. D. McIlroy, A. S. L. Malcolm, K. Playfair, H. R. Pollock, R. T. Raine, W. F. R. Schloss, J. A. B. Snell, C. R. A. Thacker, A. W. Uloth, W. T. Warwick, T. T. B. Watson, G. W. Woodhouse, R. H. Yolland, F. H. Young.

CONJOINT BOARD IN ENGLAND.

THE following candidates have been approved at the examination indicated:

SECOND COLLEGE (*Anatomy and Physiology*).—W. F. Barrett, J. Behesnilian, R. V. Bevan, A. Bishara, A. H. F. Bizarro, E. S. Bowes, I. R. R. Brogden, H. Chand, R. G. Dani, F. P. G. de Smidt, E. M. Dickson, G. B. Dowling, N. Fahmy, R. Fazan, S. M. Ghosh, E. A. Gibb, F. Y. Hassaballa, W. O. Holst, J. B. John, L. P. Johns, D. N. Kalyanvala, T. L. Kan, D. P. Karaka, T. W. Le Mesurier, N. H. S. Maelzer, F. K. Marriott, V. R. Mirajkar, E. C. de M. Morgan, F. M. Mosely, B. P. Mozoomdar, S. Muttiah, C. P. Nagamuttu, W. G. S. Neely, B. D. H. Philip, R. L. Portway, C. Y. Roberts, A. R. Roushdy, P. G. Russell, B. Sahai, J. E. Scanlan, R. J. Scarr, J. M. Smith, G. T. Symons, J. A. Tippet, M. B. M. Tweed, D. J. Valentine, H. C. C. Veitch, H. S. Wacher, L. P. Waghorn, A. Wilson, I. Zaki.

MIDDLESEX HOSPITAL.

THE competition for entrance scholarships at the medical school resulted in the award of the University Scholarship to Mr. J. J. O. Beven, of Cambridge, and of the First, Second, and Third Ordinary Entrance Scholarships to Messrs. S. C. Shaw, O. L. C. Sibley, and G. F. W. Howorth respectively. On the nomination of the head master of Epsom College the Free Lucas Scholarship was awarded to Mr. L. W. Hefferman.

ST. THOMAS'S HOSPITAL.

THE University Entrance Scholarship has been awarded to Mr. H. Monk Gould, of Charterhouse and Clare College, Cambridge.

LONDON HOSPITAL.

THE competitions for entrance scholarships at the Medical School have resulted in the following awards: The *Price Scholarship* in science (value £100) to Mr. C. S. Clarke; the *Price University Scholarship* in anatomy and physiology for candidates from Oxford or Cambridge (value £52 10s.) to Mr. J. G. Jacob, of Christ's College, Cambridge; the *Open Scholarship* in science (value £50) to Mr. G. Adler.

Obituary.

W. CARNEGIE BROWN, M.D.,

LONDON.

DR. W. CARNEGIE BROWN, whose death at the age of 54 took place on September 30th, 1913, received his early education at the Old Grammar School, Aberdeen. From there he proceeded to King's College in the same city, where he studied medicine, winning the gold medal for physiology. He also studied for a time at St. Thomas's Hospital. In the year 1880 he graduated M.B., C.M. Aberdeen, and proceeded to the M.D. in 1884; he took the diploma of M.R.C.P.London in 1906. After qualification he held various posts, and as medical officer of the White Star and P. and O. lines he visited many parts of the world, and was doubtless attracted to life abroad. He saw active service and did medical work with troops in Turkey and Egypt. In 1885 he started practice in Penang, where he continued to work until 1904. He was well known in the Straits Settlements, and held for a considerable period a seat in the Legislative Council. Like other medical men of his standing, he had not the opportunity now afforded of training in tropical medicine before going abroad, but he was a keen observer, and during his practice in the tropics he had made a careful study of the diseases with which he had to deal in the course of his work.

In 1904 he returned to England and started a consulting practice in London. Very shortly after this steps were taken to form the Society of Tropical Medicine and Hygiene, and Dr. Carnegie Brown was chosen to serve with Dr. Sandwith as one of the honorary secretaries of the new society. He threw himself into this work with great energy, and his good judgement and business capacity proved of great service to the society, which he continued to serve as secretary until the summer of 1912, when from failing health he was obliged to resign the post. He had won the respect and friendship of his colleagues of the society, and at his funeral on October 2nd there were present Sir Ronald Ross, past-president of the society; Dr. Sandwith, vice-president; Dr. Beddoes, and Dr. Harford, one of the present honorary secretaries. Dr. Sandwith also represented the London School of Tropical Medicine. A wreath was also sent by Sir Patrick Manson, the first president of the society. Dr. Carnegie Brown was the author of two important monographs on tropical subjects, one entitled *Sprue and its Treatment*, issued in 1908, and another, *Amœbic Dysentery, its Complications and Treatment*, issued in 1910. He leaves a widow, three sons, and three daughters.

EDWARD JAMES NIX, M.D.Brux., L.R.C.P.Ed.,

LONDON.

IN the passing of Dr. Nix the profession has lost not only an able member, but a representative of the highest ideal of a general practitioner. Although his loss will be most keenly felt by his friends and patients in the west end of London, Dr. Nix's practice covered an extensive area, for such was the confidence he inspired that when patients removed to a distance his services continued to be sought.

A brilliant student at Charing Cross (where he obtained medals for surgery and midwifery as well as prizes in pathology and materia medica) Dr. Nix was to the last enthusiastically devoted to his profession, keeping himself *au courant* with modern developments in spite of hard work and scant leisure. Dr. Nix's mind was active and ever on the alert; the comprehensive character of his professional knowledge was for the most part due to a

close study of his cases, never resting until he had threshed them out, and to his constant attendance at the societies. At the Medical Society in particular he was a familiar figure, where he also held office as councillor and vice-president.

Those who had the privilege of a close and lengthened friendship with Dr. Nix, as was the case with the writer of this notice, will have noted among his characteristics a high ideal of professional integrity and courtesy, a peculiar pleasure in being of service to his colleagues, a paramount concern for the interests of his patients, untiring energy, and an ever-present spirit of genial hospitality.

Dr. Nix was physician to the All Saints Children's Hospital, Margaret Street, and to the St. Elizabeth Home for Incurables. He was formerly vice-president of the Medical Defence Union, and president of the Brussels Medical Graduates' Association.

It is sad that Dr. Nix's last illness, though short, was accompanied by acute suffering, but to die in harness is just what he would have wished.

Dr. Nix has left a widow, six daughters, and one son, on whom devolves the responsibility of carrying on the practice.

C. W. C.

DEPUTY SURGEON-GENERAL NATHANIEL NORRIS, formerly of the Army Medical Department, died at Chester on September 29th, aged 82. After taking the M.R.C.S. and L.S.A. in 1854, he entered the Army Medical Department as Assistant Surgeon on April 2nd, 1855. He became Surgeon in 1867 and Surgeon-Major in 1873, retiring from the rank of Brigade Surgeon, with an honorary step, on June 24th, 1885. His war services comprised the Yowaki expedition of 1877–8, on the North-West Frontier of India, when he received the frontier medal with a clasp; the Afghan war of 1878–80, when he gained another medal; and the Soudan war of 1884–5, when he served on the Nile as Field Medical Inspector on the lines of communication, receiving a third medal with a clasp, and the Khedive's bronze star.

HIS old friends will be very sorry to hear of the death of Deputy Inspector-General JOHN LYON, M.D., R.N., which occurred on September 25th at Southport. He was born at Galston, Ayrshire, in 1847. He graduated M.B., C.M.Glasg. in 1867, and M.D. in the same university in 1876. He entered the Royal Navy in 1871 and served with the naval brigade at Magdala. He was on the flagship at the siege of Alexandria, and received the Egyptian medal and the Khedive's bronze star. He was promoted to Staff Surgeon in 1883 and Fleet Surgeon in 1891. After thirty years' meritorious service, chiefly on foreign stations, he retired with the rank of Deputy Inspector-General in 1901. After that he acted for some time in Manchester as naval medical officer for recruiting. In the later years of his life he resided at Bowdon, near Manchester, and was a member of the Altrincham Division of the British Medical Association. He was a man of very genial, kindly disposition, and will be always remembered by his old friends as Jock Lyon. He leaves a widow to mourn his loss.

IN the ninth annual report of the Department of Public Health for the year ending June 30th, 1912, it is stated that Tasmania heads the countries of the whole world in possessing the greatest relative natural increase of population, or excess of births over deaths. The high natural increase of population in Tasmania has been maintained for the years 1906–1910, the excess of births over deaths per 1,000 of mean population having been 18.37; the death-rate for 1911 was only 10.109. The infantile mortality-rate for 1911 per 1,000 births registered was 74, a reduction of 28 on the rate of the previous year. The death-rate from tuberculosis was 0.87 per 1,000 in 1910 and 0.84 in 1911. There was a gratifying reduction of the incidence of typhoid fever, from 544 in 1910 to 237 in 1911 and 152 in 1912. The excellent system of medical inspection of schools introduced by Dr. Elkington has been the means of directing attention to the inadequacy of accommodation, insufficiency of lighting, and various sanitary defects, which have been, and are, gradually being remedied.

Medical News.

THE President and Council of the Royal Microscopical Society have issued invitations to a conversazione at King's College, Strand, on Wednesday next at 8 p.m.

DR. MOTT'S address before the Midland Medical Society, Birmingham, on degeneration of the neuron in the light of recent research, will be delivered on Thursday evening, November 13th.

THE annual dinner of past and present students of the National Dental Hospital will take place at the Trocadero Restaurant on Friday, November 21st. Professor Arthur S. Underwood will be in the chair.

THE annual dinner of the Continental Anglo-American Medical Society will be held in Paris on Saturday, October 18th, at 7.30, at the Majestic, when the chair will be taken by Professor Saundby of Birmingham.

WE are asked to state that copies of the catalogue of the museum of the International Congress of Medicine, London, 1913, can be obtained by application to the Honorary Secretary of the Museum Committee (Dr. H. W. Armit, Ravenhurst, Talbot Road, Wembley), for the payment of the postage, namely, 4d. a copy.

A MEETING of the Manchester Royal Infirmary Old Residents' Club will be held at the Midland Hotel, Manchester, on Saturday, October 18th, at 6.30 p.m. The meeting will be followed, at 7 p.m., by a dinner, at which Dr. George H. Pinder will preside. Any old resident who has failed to receive a notice is requested to apply to the secretaries, Royal Infirmary, Manchester.

AN address dealing with the history of laryngoscopy from the year 1860 to 1912 will be delivered by Dr. T. J. Walker, of Peterborough, Vice-President of the Laryngological Society of London, at the Central London Throat and Ear Hospital, on Friday, October 17th, at 3 p.m., on the occasion of the opening of the winter session at this institution. The annual dinner of its staff and present students will be held the same evening, Dr. Dan McKenzie in the chair.

AT the National Dental Hospital on October 3rd, Mr. Francis R. Smyth presiding, a very handsome presentation was made to Dr. Maughan, who has been anaesthetist to the hospital for five and twenty years, of a gold watch, a solid silver rose-bowl, and a substantial cheque, in recognition of his services to the hospital, Students' Society, and *Gazette*. The gift was from the Students' Society, and subscribed by the staff and past and present students. The meeting was largely attended, and the proceedings most enthusiastic throughout.

THE annual service of the Guild of St. Luke will be held in St. Paul's Cathedral on Tuesday, October 21st, at 7.30 p.m., when the preacher will be the Rev. Canon Arnott, F.R.C.S., rector of Beckenham. The offerings, after the expenses of the service have been defrayed, will be for the missionary funds of the Guild. Tickets of admission can be obtained by sending a stamped addressed envelope to the Provost, Mr. George Cowell, F.R.C.S., 24, Harrington Gardens, S.W.

THE West London Medico-Chirurgical Society commenced the work of its thirty-second annual session at a meeting on October 3rd, when the Keetley Memorial medal was presented to the retiring president, Dr. T. P. Shuter. Subsequently the new president, Dr. F. S. Palmer, delivered an address on the subject of malingering, the psychology of which he described as a complex and interesting study. Pecuniary gain, he pointed out, was not always the underlying motive; other influences, such as idleness, resentment, or a desire for sympathy or notoriety, often played a part. The essential feature of true malingering was deliberate deceit in the direction of simulating or exaggerating the symptoms of some disease, or of misstating the date or origin of their onset, with the object of gaining some desired end.

THE winter session of the London School of Tropical Medicine was opened on October 1st with sixty-two students. The new buildings which have been provided out of the fund raised by Mr. Austen Chamberlain were opened for the first time. The new laboratory has accommodation for sixty students, and the old laboratories have been subdivided among various special departments. One of the laboratories thus provided will afford accommodation for the new course in tropical sanitation and hygiene, which will commence on October 20th. Dr. B. A. Wedd, of Guy's Hospital, has been appointed bacteriologist and demonstrator. The new residential quarters have also been occupied for the first time, the number of rooms being more than doubled, and all the accommodation provided has been already taken up. Dr. F. M. Sandwith

will preside at the annual dinner at Prince's Restaurant, Piccadilly, W., on Friday, October 24th. Old students are requested to make application for dinner tickets to the secretary at the school.

A visit to the "Ideal Home" Exhibition at Olympia on the day of the press view made it apparent that matters were still at the stage which might be described as ideal rather than real. Carpenters were busy everywhere, but officials were confident that all would be in readiness for the public opening. This is a point on which exhibition officials are always confident and on which their expectations are seldom realized. It was, however, apparent that there would be much of interest to see at a later stage. In an annexe a number of rooms were being arranged in accordance with the designs of well-known ladies, and Queen Alexandra has designed a children's day and night nursery. The fees for admission to this part of the exhibition will be devoted to the funds of the Middlesex Hospital. Further attempts to solve the problem of cheap and suitable cottage accommodation for the rural labourer find illustration in structures specially erected on the ground floor. The electric house suggests a solution of the domestic servant problem. In all parts of the exhibition are schemes of room decoration, fanciful and practical, futurist and impressionist, artistic and (to some eyes) inartistic, but all full of interest. A representation of a Russian village will doubtless attract attention; it contains many beautiful exhibits of peasant workmanship.

Among the leaflets issued recently by the Board of Agriculture is one on ropy milk. It is pointed out that ropiness in milk from individual cows, or "garget" milk, is commonly due to inflammation of the udder, and is probably to be attributed to the presence of fibrin and white corpuscles forming masses of slimy material in the milk. Though ropiness in other milk is not caused by the addition of garget milk, such milk contains organisms which may lead to infection, spreading from one cow to another. Prompt attention should therefore be given to all cows yielding garget milk, and it should not be used for food. Ropiness developing in the milk of a herd, after keeping for some time, is usually due to organisms derived from water, but sometimes from dust. These organisms are of two types—those which require a large amount of oxygen and produce either an alkaline reaction in the milk or very little acid; and, secondly, those which produce much acid, and precipitate casein; these organisms grow much better without air than the other type. The chief preventive measures recommended are that pails should not be rinsed with cold water after being scalded, and should always be kept covered to exclude dust. As a dilute solution of washing soda favours the growth of the organisms it is better to use sodium hypochlorite. As the organisms may be contained in the straw or mouldy hay, the udder should not be wiped with straw, and the milkers should not touch straw after washing their hands. Wooden vessels may retain the organism, and, if used, should therefore be kept scrupulously clean.

In the Journal of July 12th, p. 91, in commenting on the report of the medical officer of health for Johannesburg for the year ending June, 1912, we expressed the opinion that there could be no very active cause of death on the Rand—at any rate, in so far as concerns the white population. This opinion was founded partly upon the fact that the death-rate corrected for age and sex distribution amounted to only 13.3, and that the rates for typhoid fever and tuberculosis were low. The *South African Mining Review*, published in Johannesburg, has taken exception to this opinion on the ground that the cost of living is so high on the Rand that few people past their working years would live there in retirement, and that in fact most persons who are in a position to draw a pension at once leave. Our contemporary also states that the compensation paid in respect of miner's phthisis is not such as to induce sufferers to remain on the Rand. It also states that Sprinkell Sanatorium lies outside the municipal area, and assumes that the deaths occurring there are not entered in the municipality's statistics. In commenting on the report of the medical officer of health we attempted to make allowances for considerations of this kind, and we are quite prepared to believe that these factors may be somewhat larger than we had supposed; at the same time the general figures leave no doubt that, as our contemporary admits, a vast improvement must have been brought about in the hygienic conditions of the town by the work of the medical officer of health and of the town engineer, who has been most persistent in his endeavour to prevent the dust nuisance by road tarring. We altogether agree with our contemporary that the good results already achieved should only serve to stimulate further efforts in both directions.

Letters, Notes, and Answers.

ORIGINAL ARTICLES and LETTERS *forwarded for publication are understood to be offered to the* British Medical Journal *alone unless the contrary be stated.*

Correspondents who wish notice to be taken of their communications should authenticate them with their names—of course not necessarily for publication.

Authors desiring reprints of their articles published in the British Medical Journal are requested to communicate with the Office, 429, Strand, W.C., on receipt of proof.

Telegraphic Address.—The telegraphic address of the EDITOR of the British Medical Journal is *Aitiology, Westrand, London.* The telegraphic address of the British Medical Journal is *Articulate, Westrand, London.*

Telephone (National):—
2631, Gerrard, EDITOR, BRITISH MEDICAL JOURNAL.
2630, Gerrard, BRITISH MEDICAL ASSOCIATION.
2634, Gerrard, MEDICAL SECRETARY.

☞ *Queries, answers, and communications relating to subjects to which special departments of the* British Medical Journal *are devoted will be found under their respective headings.*

QUERIES.

Carfax asks if there is any institution in England or Wales where a patient whose income is £100 per annum could be treated for angina vaso-motoria.

Income Tax.

W. A. J. has a small practice from which he receives £30 after deducting his professional expenses. He is preferring a claim for repayment in respect of income tax deducted from dividends, and is informed that his profits have been assessed at £180, and that he is therefore not entitled to any repayment.

*** If "W. A. J." received formal notice of the assessment and did not lodge an objection within the time limited his right of appeal has failed. On the other hand, if he can show or if it is admitted that the notice of assessment was not served upon him, he should be able to obtain an adjustment on the ground that he had no proper opportunity of expressing his objection to the assessment.

ANSWERS.

H. M. will find that *The Examination of Waters and Water Supplies*, by Dr. J. C. Thresh (London: J. and A. Churchill, 1913, price 18s. net), contains much information likely to be useful in explaining to members of a local authority the nature of the dangers arising from contamination of the water supply.

LETTERS, NOTES, ETC.

International Medical Congress.

The Honorary General Secretary of the International Congress of Medicine, London, 1913, informs us that during the Congress a box of medals was lost, and that on this account some members of Congress or ladies may have failed to obtain them. If such persons will write to the General Secretary, 40, Wimpole Street, the medals will be sent to them.

Earth Eating and Ankylostomiasis.

Dr. J. W. Lindsay (Belén, Paraguay, South America) writes: I was interested to read the letter of your correspondent on "earth eating" in the Journal of April 5th, and have looked up your remarks on the same subject in the issue of February 1st.

As reference has been made to the prevalence of this custom in various parts of tropical South America, I may be excused if I extend these references and give my experience as to what is probably in very many cases the cause of this phenomenon.

Your correspondents have referred to it variously as a "pernicious habit," "perverted appetite," "geophagy as a disease." I am surprised that no mention has been made of what is probably the true explanation of the practice, at least in very many cases and in many parts. I do not refer at all to the eating of "sulphur earth" or calcareous or other medicinal earths, but to the eating of pure dirt or mud, black or red earth, or whatever kind of earth may be under the patient's feet when he wants to indulge his craving.

During an experience of over twelve years in the centre of South America, in which my observations have extended over the Republic of Paraguay, the adjoining Brazilian province of Matto Grosso, the northern provinces of Argentina and certain parts of Bolivia, I have found earth eating to be a symptom of a very widely-spread disease. It is true that when the natives come to consult me about it they refer to it as a "vicio," a vice or perverted appetite or pica. Their "vicio" is that they eat earth; they cannot say why they eat it. They only say they must eat earth, the craving is so

intense. I put such patients under treatment for the disease, of which the craving is but a symptom, and with the disappearance of the disease the craving immediately ceases.

About eleven years ago a European colleague who had been twenty-eight years resident in Paraguay told me that he had treated some 12,000 cases of ankylostomiasis with male fern, and that after the treatment the patients never wanted to eat earth any more. I have seen and treated hundreds of cases of ankylostomiasis in natives of all the provinces mentioned above, as well as in Europeans. They were all earth-eaters. None of them had ever lived in mining districts. They were all village people, labourers on ranches or in plantations, or members of the families of planters and ranchers. They attribute the anaemia of their disease to the poisoning by the earth they ate. There is no doubt that the ankylostoma—in South America, at any rate—requires some of the ingredients of ordinary earth in order to be able to thrive in the interior of its victims, and that the ankylostoma is the cause of the majority of cases of "earth-eating" in tropical South America.

Commode *v.* Bedpan.

DR. ARTHUR E. T. LONGHURST (Chandlersford) writes: Ever since the days of the great lawgivers the question of the disposal of human ordure, either in the field, the city, the house, or the sick room, has been a vexed one, and it is with the latter only that I am now concerned for the convenience and safety of the sufferer. After long field service and during thirty years' professional practice in London I have had considerable experience in the use of both bedpan and commode, and having recently recovered from pleurisy my personal experience and attention has again been drawn to the subject. There can be no doubt that the use of the bedpan is very disagreeable, often ineffective, and at times in many cases a very painful effort, causing the sufferer much needless exertion; and on such occasions he is much at the mercy of the nurse or other attendant, who, however careful, cannot always accurately judge the possibly undue pressure he or she may be giving to different parts of the body or its varied internal organs—in fact, the old proverb of the shoe and its pinching forcibly applies—whereas I am convinced that even in acute and enfeebled cases the sufferer will often alone, or aided by a patient and careful attendant, find the use of the commode far easier. But hurry or haste must not be permitted, the sufferer being allowed to raise himself or herself from the recumbent posture and gradually to sidle off the bed on to the commode placed at the bedside; and the return to the bed with the same care will also be easy. Too much stress, I think, in even acute cases, is often laid upon the fear of cold if the sufferer is allowed to get out of bed; but with due care, I do not think any such risk outbalances other advantages. Moreover, there can be no doubt that the position on the commode is the more natural one, and with it the sufferer can with less personal exertion aid Nature in her efforts for relief. I venture therefore to appeal to both doctor and nurse for a careful and unprejudiced consideration of the subject.

The Use and Abuse of Hypnotics.

DR. T. H. MOORHEAD (Cootehill) writes: I wish to express my entire agreement with Dr. Hardman's observation on hypnotics as well as with his criticism on current medical literature. When I take up a medical periodical I look first to see if an article on a subject that interests me has "conclusions." If it has not, I do not read the article; if it has, and any of the conclusions appear novel, I look over the article to see what arguments or facts the author advances. As for the padding Dr. Hardman refers to, I leave it severely alone, and my experience with medical friends is that frequently they ruin important matters because they have not time to separate the wheat from the—well, I won't say chaff, for it is not quite that; but life is too short to read the half of it. Dr. Hardman wonders whether the writer of the article has personal experience of the drugs he refers to. A very proper question. I recollect a junior practitioner read an article in the BRITISH MEDICAL JOURNAL on the dangers of sodium salicylate and was so scared that for years he would not give more than 5-grain doses even in acute rheumatism. Since then he has learnt to rely upon his own judgement. Talking of these bogeys, I wonder how long it will take to explode the fallacy passed on from one generation of students to another that opium preparations are dangerous to children, and that mercurials should not be given to patients with renal disease, and others of the same class. I was taught them nearly half a century ago, and still they crop up as fresh as ever.

Coincidence or Telepathy?

DR. B. JONES (Boscombe) writes: The communication under this heading by Dr. Phillips is certainly an instance of coincidence, scarcely more. The wonder is that instances do not oftener occur of this character. On the contrary, how often has one seen people in high spirits when some untoward event has been taking place in some member of the family. Those who infer from happenings such as this that there is some mysterious influence at work which they call telepathy or some other weird name, overlook the many times that people have a foreboding of disaster which never takes place. I remember on one occasion having a foreboding of this kind with regard to a favourite old dog of my wife's. We had had a very pleasant holiday; but as we approached home in the train I had a very distinct feeling of impending disaster. I felt certain that our old favourite had died, and I was apprehensive as to the effect it would have upon my wife, who was deeply attached to it. My first inquiry was about it, never expecting but one answer. Much to my relief and surprise I was told that it was quite well, and in a few minutes I found it in its usual health, and overjoyed at our return. It lived for years afterwards. So with dreams. There are a few startling instances on record of "coming events casting their shadows before." But is not it a greater wonder that with so millions of people dreaming every night of all sorts of things, their dreams mostly influenced by externals, whether of newspapers or books or their personal affairs, that really so few shape events as they really happen. If one could have well-authenticated cases where every detail as to time and circumstances were exactly as happened afterwards or before, if beyond one's ken, one might call it extra-physical. At present there are very few such records.

Old Coffees and Young Teas.

DR. CHAS. MERCIER (Parkstone, Dorset) writes: "Old coffees, young teas" you quote (p. 819) from an anonymous authority, but the maxim was not always allowed. In 1769 Dr. Lettsom published "an inaugural dissertation intituled 'Observationes ad Vires Theae pertmentes,'" of which he published in 1772 an English translation with additions. In this he states that "neither the Chinese, nor the natives of Japan, ever use tea before it has been kept at least a year; because when fresh it is said to prove narcotic, and to disorder the senses." He gives his authorities in a footnote, which I transcribe—Kæmpfer, *Amoenit. Exot.*, p. 625. *History of Japan*, vol. ii, App. p. 10, 16. I am not in a position to refer to the originals, but Dr. Lettsom was an accurate man, and no doubt his transcription is correct. It would be interesting to know from those who have resided in China and Japan whether the practice is still followed. On the same page he says:

"The Japanese reduce the tea into a fine powder, by grinding the leaves in a handmill; they then mix them with hot water into a thin pulp, in which form it is sipped, particularly by the nobility and rich people. This is called 'koitsjœa,' that is, thick tea, to distinguish it from that made by infusion."

What do those who have been entertained in recent years by the nobility and rich people of Japan say about this?

Monuments in Winchcombe Church.

Dr. WILLIAM MILLIGAN (Hanwell, Oxon.) writes: In the fine old parish church of Winchcombe, Gloucestershire, are two seventeenth century monuments the inscriptions on which are of interest to the members of the medical profession, and which, I think, have not previously been noticed in the JOURNAL:

> Hic jacet Elizabetha uxor amicissima Gulielmi Harvey clerici. Triduo labore defessa obstetricantis manus vix et ne vix quidem auxilio tertiam enixa sobolem.

The Rev. John Taylor, B.D., D.Lit.Lond., lately vicar of the parish, gives the English version of the most interesting part: "Worn out with three days' labour she hardly and not indeed hardly brought forth her third child with the help of the midwife's hand." The inscription on the other monument runs as follows:

> Hic etiam jacet idem Richardus, Caelebs (si quis alius) vitae integrae, notusque inter suos animi paterni.
> Obiit xix Martii, Anno Aerae Christianae mdclxx, Aetatis lxiv.
> Honoris gratitudinisque causa hoc addidit Christophorus Merrett Nepos.

Dr. Taylor writes to me: "It is most probable that the C. Merrett who dedicated this brass to the memory of Richard, a bachelor (if ever there was one) of upright life, etc., was the famous Ch. Merrett, first custodian of Harvey's library and museum, one of the earliest Fellows of the Royal Society."

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